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The American Historical Review

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Science and Values: The Eugenics Movement in Germany and Russia in the 1920s

LOREN R. GRAHAM

THEORETICAL DISCUSSIONS OF THE RELATIONSHIP between science and values usually lead to the conclusion that, in a strict sense, science is value-free. If both the impact of technology on values and the influence of scientists as a political and social group are excluded from consideration, a persuasive case can be made that science is, indeed, neutral. There is no logical bridge between "is" and "ought." But this form of exclusivist analysis of science overlooks some of the most important aspects of the relationship between science and modern culture. Explicitly, these examinations exclude links between science and technology as well as connections between societal values and conditions for scientific research. Implicitly, these discussions exclude those arguments linking science to social value that are the most common in our imperfect world: the incomplete, semiscientific, possibly fallacious claims that are often advanced in the name of science and the evaluations of these claims that every citizen has to make in order to decide about the relevance of science for social and political life. Two separate but chronologically simultaneous episodes in the history of human genetics involve all of these "second-order" links between science and values: the development in the 1920s of eugenic movements in Germany and Soviet Russia. Comparing them produces unusual insights into the connections between science and political values.

Mention of the topic of human genetics in Germany and Russia in the early twentieth century automatically brings to mind the issues of National Socialist racial theories in Germany and Lysenko's theories of heredity in Soviet Russia. In the period covered here, however, National Socialism had not yet come to power in Germany and scientists of greatly differing political views—including Marxists, socialists of various sorts, Catholics, liberals, and conservatives—discussed human genetics. In Soviet Russia in the same years no one had yet heard of Lysenko, and the spectrum of debate about human

I would like to express my appreciation to the Rockefeller Foundation for a Humanities Fellowship for 1976–77, during which time I did research on the relationship of science and sociopolitical values, including this article. I would also like to thank Harvard University for providing research facilities during the year, particularly the Program on Science and International Affairs, the Department of History of Science, and the Russian Research Center. A shorter version of this article will appear in *Knowledge, Value, and Belief*, edited by Daniel Callahan and H. Tristram Engelhardt, Jr. and published by the Institute of Society, Ethics, and the Life Sciences, Hastings-on-Hudson, New York.

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genetics was surprisingly broad; there were Marxists who were also eugenists and saw no contradiction in their respective positions, and there were anti-Marxists who were Lamarckians. In both Germany and Russia in the 1920s, then, the discussions about human heredity and its social implications preceded the period in which single-minded political concerns overruled scientific judgment. How did it happen that this early complex period in which so much uncertainty existed about the political value implications of theories of human heredity was replaced in both countries by a later period in which Mendelian eugenic theories were usually linked to conservative views of society and Lamarckian theories were usually linked to left-wing socialist views of society? Was this process entirely a social and political phenomenon, essentially distinct from the scientific theories under discussion, or was there something intellectually inherent in each of the competing theories of heredity which supported a particular political ideology? If the answer to the latter question is affirmative, what do these inherent factors say about the allegedly value-free nature of science?

IN WEIMAR GERMANY AND SOVIET RUSSIA the peak of the debates over the social and political implications of theories of human genetics occurred in the mid-1920s. Scientists in each of these countries were aware of ongoing debate in the other. In both instances, however, the debates occurred within unique contexts formed by the recent histories of the two nations. In Germany, as in many other countries, a great debate had occurred over Darwinism and Social Darwinism in the last part of the nineteenth century.[1] Many of the views expressed there were similar to those uttered elsewhere, but several features were stronger in Germany and deserve particular attention. One characteristic of German Social Darwinism was a gradual decline in the last decades of the century in the optimistic variation of Social Darwinism and a growth of the pessimistic one. The optimistic variation is that interpretation of Social Darwinism which sees in the economic competition of capitalism an analogue to biological competition and postulates a direct benefit for the most creative and successful entrepreneurs and an indirect benefit for society as a whole. This form of Social Darwinism is so well known that it need not be discussed here; it was particularly popular in England and the United States. The reasons for its relative decline in Germany at the end of the century are complex. The general tendency toward cultural pessimism and discontent with rapid industrialization among educated classes often noted by historians

[1] There is a very large literature on Darwinism and Social Darwinism in Germany. See, in particular, Hans-Günter Zmarzlik, "Der Sozialdarwinismus in Deutschland als geschichtliches Problem," *Vierteljahrshefte für Zeitgeschichte*, no. 11 (1963): 246–73, and "Der Sozialdarwinismus in Deutschland—Ein Beispiel für den gesellschaftspolitischen Missbrauch naturwissenschaftlicher Erkenntnisse," in *Kreatur Mensch: Moderne Wissenschaft auf der Suche nach dem Humanum*, ed. Günter Altner (Munich, 1973), 289–311. Also see William M. Montgomery, "Germany," in *The Comparative Reception of Darwinism*, ed. Thomas Glick (Austin, Tex., 1974), 81–116; Gerhard Heberer and Franz Schwanitz, eds., *Hundert Jahre Evolutionsforschung: Das wissenschaftliche Vermächtnis Charles Darwin* (Stuttgart, 1960); and Hedwig Conrad-Martius, *Utopien der Menschenzüchtung: Der Sozialdarwinismus und seine Folgen* (Munich, 1955). For interesting but somewhat simplified accounts, see Daniel Gasman, *The Scientific Origins of National Socialism: Social Darwinism in Ernst Haeckel and the German Monist League* (New York, 1971); and Niles R. Holt, "Monists and Nazis: A Question of Scientific Responsibility," *The Hastings Center Report*, 5 (1975): 37–43.

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undoubtedly played a role. Yet other influences are also evident: (1) the socialist critique of unfettered capitalism; (2) the popularity, even among the petty bourgeoisie and workers, of Social Darwinist views in the form presented by Ernst Haeckel and the monists; and (3) the strength of theories of an independent, immutable germ plasm, particularly the one promoted by August Weismann. Socialism helped to convince a large portion of the German population, including many who were not socialists, that unrestrained capitalism was not appropriate to Germany. By the end of the century Germany had the largest socialist party in the world and the most extensive set of social welfare laws mitigating the effect of capitalist competition. Monism prepared a broad segment of the German population for biological arguments applied to society. And germ-plasm theory gave heuristic clarity—especially important to lay audiences—to the concept that each individual is a genetic custodian with the responsibility for preserving his or her own genetic "package" for the future. If those with the "best" genetic heritages handed them on to the next generation less frequently than others, the overall result for a population would be genetic degeneration.

The transition from optimistic to pessimistic forms of Social Darwinism can be seen in the contrast between two early German Social Darwinists, Otto Ammon and Wilhelm Schallmayer. In 1895, Ammon, an engineer and private scholar, published *The Social Order and Its Natural Foundations*, in which he maintained that natural selection among humans was the key to social progress. He tried to humanize this conclusion and to reconcile his Christian ethics with economic competitiveness by asserting that there are two kinds of natural selection: the one individualistic and egoistic, the other altruistic and social. Ammon looked upon this automatic and natural process with awe, seeing in it a secret mechanism beyond man's complete comprehension. Nature was superior to man and therefore any attempt by man to help the process along through eugenics was certain to go awry. "We must not attempt to breed men selectively as if they were cattle!" he angrily exclaimed. He considered it ridiculous to believe that "mere humans, especially small-spirited ones, who have never understood the iron law of heredity, might interfere with bold hands in order to improve it and remake it from the ground up."[2]

In contrast to Ammon, Schallmayer exhibited a pessimistic interpretation of Social Darwinism. Schallmayer in 1900 won first prize in a writing competition established by the German industrialist Alfred Friedrich Krupp on the topic, "What can we learn from the theory of evolution about internal political development and state legislation?"[3] In his award-winning book, Schallmayer observed that "the possibility of genetic progress stands face-to-face with the possibility—in case of inadequate selection or direct germinal impairment—of genetic regression, which can go so far in a relatively short period of time that it can pull down political power with it."[4] According to Schallmayer, those interpreters of Darwin who saw only progress in his teach-

[2] Ammon, *Die Gesellschaftsordnung und ihre natürlichen Grundlagen* (Jena, 1895), 25, 26.

[3] See the description in Conrad-Martius, *Utopien der Menschenzüchtung*, 74–75.

[4] Schallmayer, *Vererbung und Auslese im Lebenslauf der Völker: Eine staatswissenschaftliche Studie auf Grund der neueren Biologie* (Jena, 1903), 100.

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ings were simply misreading him, since the story of evolution is replete with episodes of extinction. Schallmayer feared that man's emphasis on individual, rather than group, interests was bringing him into similar danger. By developing a system of ethics based on the rights of the individual, civilization had actually done a disservice to the human race, since man was no longer willing to sacrifice the individual for the sake of community welfare. To Schallmayer, Weismann's view of autonomous germ plasm graphically illustrated that what mattered from the standpoint of modern biology, was that individuals secure the preservation of the species through proper transmission of their genetic heritage (*Erbgut*), not that man make more comfortable either his own temporal existence or that of his fellow man. In fact, more comforts and better social conditions might actually be detrimental to the future of the human race. Degeneration (*Entartung*) was more of a danger among civilized peoples than among primitive ones because social measures such as those already prevalent in Germany interfered with the "natural" process of selection.[5] Conquering disease through medicine, the care of the incapable and the unfit by social security, and the spread of contraception among the educated all had the effect of lowering the quality of the genetic heritage passed on to successive generations.

Although he seems to prefigure the extremes to which eugenic and racist doctrines later deteriorated in Germany, Schallmayer actually considered himself a radical democrat and in subsequent years stubbornly opposed the National Socialist interpretation of race. He agreed that life selection (the elimination of the unfit) occurs in nature, but he considered it totally impermissible among humans. Rather, he hoped for a system of voluntary eugenics, in which individuals would, through education, make careful decisions about the desirability of having large numbers of children. He insisted upon humane methods, and he saw no correlation between a biological interpretation of society and right-wing politics. Indeed, he considered eugenics a progressive doctrine, as did several political groups in other countries during the first years of the movement.[6]

The degree to which early German eugenists at first separated themselves from particular political viewpoints can be seen in the initial writings of Alfred Ploetz, a founder of the leading German eugenics journal and a central figure in the German eugenics movement of the 1920s. As a young man Ploetz's interest in the genetic future of man was stronger than his commitment to any particular social or economic order, and he criticized both capitalism and socialism for the contradictions which he saw between them and the ideals of eugenics. Ploetz, therefore, was no businessman or academic scholar defending economic competition on the basis of Darwinism, a model long familiar to us; rather, he wanted to find a third way, a eugenic society that would put the

[5] *Ibid.*, esp. chap. 7, 174–211.

[6] For the United States, for example, see Rudolph Vecoli, "Sterilization: A Progressive Measure?" *Wisconsin Magazine of History*, 43 (1960): 190–202; and Donald K. Pickens, *Eugenics and the Progressives* (Nashville, 1968).

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good of the future above the comforts of the present. Socialism presented the prospect of a humanitarian society through the alleviation of economic suffering. But socialism thoroughly contradicted the principles of eugenics. If the "weak" were protected through social legislation guaranteeing employment, health, and life support, then they would propagate their kind in increasing numbers. Capitalism was no better. It caused great human suffering—Ploetz cited Karl Marx, Karl Kautsky, August Bebel, and other socialists to that effect. And, furthermore, by giving advantages to the hereditary wealthy in the struggle for existence, it hindered free natural selection.[7]

One might erroneously suppose from this analysis that the most appealing form of society to Ploetz was a somewhat softened capitalism, in which individual competition would still play the major role in determining success in any one generation, but in which inherited wealth and privilege would be prohibited. In such a society each generation would have to prove itself anew; the losers would fall to the bottom of the heap, where they would be protected from the most extreme forms of suffering. Ploetz, however, was both too complex and too humane to propose such a society. He seemed to favor all aspects of socialism except its alleged genetic consequences, and the specter of open competition within each generation was offensive to him. Therefore, he struggled to find a way of combining the humanitarian and egalitarian ideals of socialism with the eugenic goal of a constantly improving biological base for society, since the potential unification of the principles of eugenics with "humane socialist principles is of the utmost significance for the further evolutionary development of human society."[8]

Ploetz believed that he had found a way to reconcile his contradictory demands. He proposed that all people be protected by welfare legislation, as the socialists demanded, from the raw struggle for survival inherent in pure capitalism; but at the same time he wanted to ensure that each generation consisted of genetically healthy individuals by having each married couple select the "best" of their own germ cells for fertilization. Ploetz noted that the germ cells, or gametes, of each couple vary greatly in their genetic constitution; from the standpoint of eugenics, the children needed to be at least genetically equal to their parents. By "shoving selection back" to the prefertilization stage he hoped to achieve eugenic goals without tampering with individual human rights or interfering with natural, legal parenthood. "The more we can prevent the production of inferior variations," he wrote, "the less we need the struggle for existence to eliminate them. We would not need it at all if in each generation we were able to ensure that the totality of variations was qualitatively somewhat superior, on the average, to that of the parents."[9] Ploetz's suggestion was naive and entirely impractical from the standpoint of the biological science and technology of his time. No one in the early years of this century had the faintest idea how to give an overall evaluation of the

[7] Ploetz, *Die Tüchtigkeit unserer Rasse und der Schutz der Schwachen: Ein Versuch über Rassenhygiene und ihr Verhältnis zu den humanen Idealen, besonders zum Socialismus* (Berlin, 1895).

[8] *Ibid.*, 212. [9] *Ibid.*, 224–25.

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genetic quality of different gametes (nor can it be done now). In stating the problem of a clear and pressing threat of degeneration on the basis of quite inconclusive evidence and proposing as a solution a scheme which was wildly futuristic, Ploetz actually solved nothing. In addition, he left the road clear for less humane schemers. Furthermore, Ploetz continued to support the German eugenic movement as it deteriorated, abandoning his earlier humane principles.

Nonetheless, Ploetz made an important point which is more interesting to us now than it was at his time. The parents who today decide to abort a fetus found to be mongoloid through amniocentesis are following Ploetz's policy of "shoving selection back" to the prebirth stage in the name of humane principles, although not to the gametes themselves. And a few recent efforts have been made even to extend selection back to the gametes, as Ploetz suggested, not only for the determination of sex but also for other specific genetic reasons. Controversial as some of these proposals are, Ploetz was prescient in seeing that value consequences of applying the science of human heredity depend more on *when* and *how* it is applied than on the question of what is inherent in the science itself. It would, of course, be a mistake to draw a close parallel between Ploetz's goals and those of contemporary genetic counseling based on amniocentesis. Ploetz worried primarily about avoiding an overall decline in the quality of future generations viewed collectively, a worry that we now know he grossly exaggerated; he was not primarily concerned with avoiding a genetic flaw and the consequent suffering for one child and its family. Ploetz's significance, then, lies not in the similarity of his situation to ours, but his early effort to analyze the differing social value consequences of different approaches for applying the science of human heredity.

IN THE MID-1920S ACADEMIC GENETICISTS, anthropologists, and eugenists in Germany disagreed sharply even over the terminology of human heredity and race. Although these disputes seem somewhat scholastic at first glance, they formed the essential theoretical background for the subsequent degradation of the science of human heredity in Germany. What occurred in this period was the gradual crystallization of political value links to specific biological interpretations.

One of the issues that divided German specialists in human heredity was the appropriate German word used to translate the English term "eugenics"—a word devised by Francis Galton in 1883. At first the analogous German word "*Erbhygiene*" was preferred; later, the more direct "*Eugenik*." The socialist Alfred Grotjähn, who strongly favored eugenic measures to avoid what he saw as imminent degeneration, employed the term "*Fortpflanzungshygiene*," a word he thought accurately described his concerns, while showing that he rejected the racist interpretation of eugenics.[10] By the 1920s

[10] See Lenz's review of Grotjähn's *Leitsätze zur sozialen und generativen Hygiene* in *Archiv für Rassen- und Gesellschaftsbiologie*, 15 (1923): 89–91. Also see Karl Kautsky's review of Grotjähn's *Die Hygiene der menschlichen Fortpflanzung: Versuch einer praktischen Eugenik* in *Die Gesellschaft*, 4 (1927): 381–82.

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the term "*Rassenhygiene*" had gained in popularity, although its exact meaning remained in dispute.[11] Did "race hygiene" apply to one human race? If so, some people could maintain that in itself the term did not connote racial differentiation. Or did it mean "races" and a consequent hierarchy of racial worth—the definition ever more widespread in right-wing political literature? Some German eugenists, including Wilhelm Schallmayer, preferred "*Rassehygiene*"; the omitted "n" indicated that they were speaking of only one race, the human species. Others said the word "*Rassenhygiene*" was acceptable because the middle "n" was not a plural but a variation of the genitive singular.[12] Still others insisted that the "n" was, indeed, a plural form and that a differentiation of races on the basis of genetic "worth" was an entirely scientific goal.[13]

The word eugenics today is usually considered a pejorative term and is even, on occasion, erroneously equated with National Socialist doctrines, but in Weimar Germany "eugenics" was thought of by proto–National Socialist publications and organizations as a kind of leftist deviation. Race hygienists carefully followed the eugenics movements in other countries and frequently pointed out that the leaders of these movements there often failed to understand the racial significance of eugenics. Whether one used the term "*Rassenhygiene*" or "*Eugenik*" became in the late 1920s a kind of political flag, often with the more right-wing members of the movement favoring the first term, the more left-wing members the latter. In the last years of the Weimar period, alarmed by the growing racist sentiment among specialists in human heredity and their increasing alliance with the political right wing, a coalition of Centrist, Catholic, and Social Democratic biologists and anthropologists mounted a counterattack within the German Society for Race Hygiene with the goal of changing its name to the German Society for Eugenics. They received important support from Eugen Fischer, a prominent anthropologist, who, according to a recent historian of German anthropology, was still trying to reconcile his concept of human genetics with his Catholic faith. In later years Fischer ignored the pope's encyclical condemning eugenics, abandoned his religious hesitations, and joined fully with the race hygiene movement.[14]

[11] Alfred Ploetz proposed the term *Rassenhygiene* in 1895. He was not certain whether or not it was a new term. As he observed, "Ich weiss nicht, ob das Wort Rassenhygiene schon ausgesprochen wurde oder nicht; sicher ist, dass der darin enthaltene Begriff längst in vielen Köpfen lebte, und dass er in den Geisteskämpfen unserer Tage eine grosse Rolle spielt." *Die Tüchtigkeit unserer Rasse*, 5.

[12] Alfons Fischer also insisted on spelling the word "*Rassehygiene*." See Fritz Lenz's review of Fischer, *Grundriss der sozialen Hygiene* in *Archiv für Rassen- und Gesellschaftsbiologie*, 8 (1926): 228–30. Also see the review of W. Schallmayer, "Einführung in die Rassehygiene" (*sic*) in W. Weichardt, ed., *Fortsetzung des Jahresberichts über die Ergebnisse der Immunitätsforschung*, in *Archiv für Rassen- und Gesellschaftsbiologie*, 13 (1918): 105–07.

[13] The emerging demagogic element can be seen in Lenz's statement: "Wenn man schon Rücksicht auf politische Richtungen nimmt, so muss man allerdings auch bedenken, dass für die volkische Bewegung, die in Deutschland auch eine Macht darstellt, und zwar eine wachsende Macht, das Wort Rasse eine angenehmen Klang hat. Gerade mit Rücksicht auf die Aussichten praktischer Durchführung rassenhygienischer Reformen tut man daher meines Erachtens nicht gut, das Wort Rassenhygiene ganz zu vertilgen." As quoted in Günter Altner, *Weltanschauliche Hintergründe der Rassenlehre des Dritten Reiches: Zum Problem einer umfassenden Anthropologie* (Zurich, 1968), 38.

[14] K. Saller, *Die Rassenlehre des Nationalsozialismus in Wissenschaft und Propaganda* (Darmstadt, 1961), 16–17, 72. In his 1930 encyclical "On Christian Marriage," Pope Paul XI went beyond condemnation of contraception and abortion to a specific castigation of all those who "put eugenics before aims of a higher order, and by public authority wish to prevent from marrying all those whom, even though naturally fit for marriage, they consider, according to the norms and conjectures of their investigations, would, through hereditary transmission, bring forth defective offspring." "Encyclical Letter of his Holiness Pius XI by

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For the moment, however, Fischer aided the rebels combatting race hygiene. The campaign did not succeed, since it resulted in a weak compromise on the title "German Society for Race Hygiene (Eugenics)." A few of the rebels believed they had shown that *Rassenhygiene* was simply a German word for eugenics with no necessary racist connotations.

If Catholics frequently had reservations about eugenics, socialists and Communists objected to the racist doctrines contained in the race hygiene movement, even if they accepted eugenics. Their objections, however, had quite different roots. The Catholic Church wanted to maintain, not degrade, Christian ethical values and preserve, not destroy, the family as an institution. The political leftists had no such concerns, but they saw and denounced the growing alliance in race hygiene circles between anti-Semitism and right-wing politics. The position of the German socialists and Communists toward eugenics in the twenties was complex and interesting. Contrary to what we might assume, in the early and middle years of the decade the leftist press did not define the debate in terms of nature and nurture where nature was politically conservative. The leftists noted that many of the eugenists opposed the traditionally conservative institutions of society—monarchy and nobility—on the grounds that they were genetically regressive, without correlation between influence and natural ability. And most Marxists prided themselves on their scientific outlook and regarded knowledge of genetics as desirable. Furthermore, they, like the eugenists, definitely favored societal reform. Therefore, many socialists and Communists supported eugenics.

One area where eugenic considerations—and the type of social analysis used by eugenists—early entered into the discussions of the Social Democrats was abortion. In the 1920s the proposal to permit legal abortion created great controversy in Germany and Austria, and socialists seriously divided over the issue. They universally condemned the class consequences of the existing system, in which abortions were illegal but widespread: the rich obtained adequate services while the poor were condemned to frightful conditions. Furthermore, the Social Democrats usually favored freeing abortions from religious and absolutist restrictions, but they questioned the degree to which they, as socialists, should regard abortion as an individual decision, as if the fetus were private property, and the extent to which abortion had to be a societal decision. Karl Kautsky, Jr. wrote that, after long pondering this question, he had decided his earlier support of the individual decision was "unsocialist"; the stake of society in the number and quality of children to be born was too high to allow pregnant mothers unrestricted right to control their own bodies. He noted that in 1924 the Organization of Social Democratic Physicians in Vienna had recommended against making abortion a completely free individual decision and had, instead, called for group decisions to be made jointly for each abortion by physicians and public committees, which

Divine Providence Pope, On Christian Marriage," National Catholic Welfare Conference, Washington, D.C., 1931, 23–24. Eugen Fischer became the rector of the University of Berlin after the National Socialist takeover.

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would base their decisions on "medical, social, and eugenic" indicators.[15]

The principle of responsibility to society as a whole lay at the basis of many early eugenic and race hygiene programs. The Democratic Socialist writers of the 1920s often applauded this principle even when it was advanced by racist authors. In 1923, writing in one of the oldest socialist journals, the *Socialist Monthly*, George Chaym observed, "Socialism certainly does not take a negative position toward race hygiene [*Rassenhygiene*], insofar as race hygiene concerns theoretical and practical measures for the improvement of the race or avoiding its debasement [*Verschlechterung*]." Chaym continued that, correctly understood, race hygiene "unconditionally belongs in socialism's work program."[16] The most prestigious journal of German social democracy in the 1920s, *Die Gesellschaft*, contains numerous discussions of race hygiene and eugenics. Up to the end of the Weimar period the journal continued to support eugenics when it was divorced from extremist and racist interpretations.[17] Even the word "*Rassenhygiene*" continued to be used by many leftist writers. In 1926 Oda Olberg, in a review of *Biological Problems of Race Hygiene and Cultured Peoples*, called for "a more careful consideration of race hygiene requirements by socialists and other people in Social Democratic-oriented endeavors." In another issue the same author observed, "I am not able to understand why the question of the protection of our racial value [*Rassenwerte*] should be looked at through one or another particular pair of partisan, political, religious, or philosophical spectacles. This problem stands higher than any kind of party politics."[18]

In June 1926 Kautsky wrote a long review of Olberg's own book, *Degeneration and Its Cultural Conditioning*, a volume that contained a particularly clear exposition of the view that civilized man was genetically declining because of the softening of selection by social welfare measures. Olberg could not be dismissed as a right-wing racist fanatic; she was a loyal Social Democrat who had often written for *Die Gesellschaft* in the past. Furthermore, she stoutly defended Democratic Socialism in her work, including its social welfare program; she claimed that only when class differences had been eliminated from society could the environmental and genetic causes of social ills be separated. Once that was accomplished, she was convinced that socialism would have to adopt a eugenic program. Kautsky praised Olberg for raising these issues, but he was clearly worried about her analysis. For all of his own reverence for science, he saw that Olberg exaggerated its significance for

[15] Kautsky, "Unterbrechung der Schwangerschaft: Individualistische oder sozialistische Lösung?" *Die Gesellschaft*, 2 (1925), 354–66.

[16] Chaym, *Sozialistische Monatshefte*, 10 (1923): 638.

[17] In addition to the articles and reviews specifically discussed in the text, see the following reviews in *Die Gesellschaft*: R. F. Fuchs of A. Basler, *Einführung in die Rassen- und Gesellschafts-Physiologie*, 3 (1926): 283–86; Karl Kautsky of Alfred Grotjähn, *Die Hygiene der menschlichen Fortpflanzung: Versuch einer praktischen Eugenik*, 4 (1927), 381–82; M. Kantorowicz-Kroll of Robert Sommer, *Familienforschung, Vererbungs- und Rassenlehre*, 5 (1928): 92–94; and Miron Kantorowicz of Ernst Neumann, *Individual-Rassen- und Volkshygiene*, 8 (1931): 288.

[18] Olberg, review of Stavros Zurukzoglu, *Biologische Probleme der Rassenhygiene und die Kulturvölker*, in *Die Gesellschaft*, 3 (1926): 182, and review of von Bohr-Pinnow, *Die Zukunft der menschlichen Rasse: Grundlagen und Forderungen der Vererbunglehre*, in *Die Gesellschaft*, 3 (1926): 484.

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politics. He questioned her main thesis—the belief that an increase in culture automatically brings with it an increase in genetic degeneration. He was bothered by her lumping so many different phenomena together as examples of degeneration—proliferation of crime, decrease in nursing by mothers, increase in mental illnesses, and so on. And he expressed fears about what the acceptance of such views by Social Democrats would do to their political programs. But he agreed with Olberg that eugenic concerns are important to society and should form a part of the political consciousness of Social Democrats.[19]

The first major criticism of the race hygiene movement to appear in *Die Gesellschaft* was an article in February 1927, by Hugo Iltis, a biographer of Gregor Mendel. In this article, which revealed a great deal about socialist attitudes toward eugenics, Iltis was directly critical of the majority of German race hygienists. He said they subverted science for politics and noted with regret the presence among them of a number of prominent academic geneticists. He criticized the typological description of races, which—displacing the populational view—attributed mental and ethical qualities to individual races.[20] He also saw clearly the dimensions of the threat; indeed, he said that "delusion about race is on the point of conquering German science." And he predicted that "the time will come when the race hatred and race obscurantism of our day will be what witchcraft and cannibalism are for us now: the sad vestiges of depraved barbarism." Surprisingly, however, not only did Iltis continue to support eugenics, but he even accepted the term "*Rassenhygiene.*" He was convinced that the main problem of eugenics was its drawing of premature and inhumane conclusions on the basis of fragmentary data. In the future, he maintained, after "decades and perhaps centuries of hard work," we will create "the foundations of a genetic race hygiene," one which will serve the whole human race.[21]

Iltis also saw in Lamarckism a way of softening the hard facts of human genetics, and in that way he helped forge the links between leftist politics and Lamarckism that were growing in the 1920s. He believed that only through the inheritance of acquired characteristics would it be possible to show that everything the socialists were working for—better education, better living conditions—would have a beneficial genetic effect. As he observed, "once Lamarckism has found its great methodizers and as much time is given to its study as is given today to Mendelism, it will be possible to find the means by which not only the individual human races but the human race as a whole can be raised to a higher level, and its physical, ethical, and emotional qualities can be advanced."[22] Thus, Iltis held out an unattainable, scientifically unjustified ideal: a Lamarckian eugenics directed toward the genetic betterment of

[19] Kautsky, review of Olberg, *Die Entartung in ihrer Kulturbedingtheit: Bemerkungen und Anregungen*, in *Die Gesellschaft*, 3 (1926): 567–73.
[20] Iltis, "Rassenwissenschaft und Rassenwahn," *Die Gesellschaft*, 4 (1927): 97–114.
[21] *Ibid.*, 108, 114, 113. [22] *Ibid.*, 113.

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mankind, a science that would grant a place to both Mendelian and Lamarckian inheritance.[23]

Not all left-wing German eugenists supported Lamarckism, but the conservative race hygienists were delighted when they could hang the label of "Lamarckism" on their opponents; critics like Iltis became easy game. One of the leaders of the German race hygienists, Fritz Lenz, who applauded Hitler even before he came to power, retaliated for the right-wing movement by dismissing the most prominent socialist and religious objectors to race hygiene as hopeless Lamarckians trying to dodge the facts of modern biology by creating the fiction of the inheritance of acquired characteristics.[24] Lenz was an academically qualified geneticist, and on the question of Lamarckism he exploited his advantage to the hilt. He pointed out that Paul Kammerer, a dedicated socialist and admirer of the Soviet Union, was trying to prove the inheritance of acquired characteristics with his experiments on the midwife toad. The experiments were exposed as fraudulent, after which Kammerer committed suicide. In the Soviet Union, Kammerer was made a hero, the subject of a popular film.

Lenz described all of this in his journal *Archive for Race and Social Biology*, and then turned the tables on his left-wing critics. "They accuse us," he said, "of inserting our own values into biology, when actually they are the ones guilty of maintaining that biology contains humane principles, those of Lamarckism." "*We* know, on the contrary, that science is value-free," he continued. "We must follow the facts of human heredity wherever they take us, and those facts tell us that man will genetically degenerate unless the strong and the fit are given advantages in propagation." He declared that the inheritance of acquired characteristics is a myth and that Mendelian laws alone govern human heredity.[25]

Perhaps the most outspoken attack on the German race hygiene and eugenics movement to appear in the entire Weimar period was an article in 1928 in the Communist journal *Under the Banner of Marxism*. In it Max Levien presented a classical Marxist critique, simplistic but trenchant, and he avoided the Lamarckian trap. The central thesis of his long polemic postulated that the main current of German race hygiene was a bourgeois science which served the ruling class of German capitalists. The leading race hygienists preached a false theory of genetic degeneration because, by portraying

[23] Iltis continued his strong opposition to National Socialist views on race. See, for example, his "Der Schädelindex in Wissenschaft und Politik," *Die Gesellschaft*, 8 (1931), 549–62.

[24] For examples of Lenz's attacks on Lamarckism, see his reviews of Hermann Paull, *Wir und das kommende Geschlecht*, in *Archiv für Rassen- und Gesellschaftsbiologie*, 15 (1924): 330–32; of Wilhelm Schmidt, *Rasse und Volk*, in *ibid.*, 21 (1928): 111–15; and of Friedrich Hertz, *Rasse und Kultur*, in *ibid.*, 18 (1926): 109–14. An example of Lenz's praise of Hitler before the National Socialists came to power is the following 1931 statement: "Hitler ist der erste Politiker von wirklich grossem Einfluss, der die Rassenhygiene als eine zentrale Aufgabe aller Politik erkannt hat und der sich tatkräftig dafür einsetzen will"; see Lenz, "Die Stellung des Nationalsozialismus zur Rassenhygiene," *Archiv für Rassen- und Gesellschaftsbiologie*, 25 (1931): 300–08.

[25] Lenz, "Der Fall Kammerer und seine Umfilmung durch Lunatscharsky," *Archiv für Rassen- und Gesellschaftsbiologie*, 21 (1929): 311–18. Lenz repeatedly made the argument that science was value-free. In 1921, for example, he said, "Die Natur verlangt überhaupt nichts; die Naturwissenschaft kann nur zeigen, was geschieht, nicht was geschehen soll." *Archiv für Rassen- und Gesellschaftsbiologie*, 13 (1921): 112.

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such a threat, they hoped to justify imperial expansions against "inferior races and nations," particularly the Eastern Slavs. Both of these goals were parts of their program of stemming the revolution already visible in the East.[26] Like the Democratic Socialists, the Communist Levien believed that eugenics was a progressive, useful science. Once the revolution was victorious, eugenics could serve the proletariat as it had once served the capitalists. Levien said that German Marxists must not let the race hygienists do damage to "serious scientific efforts to create a people's eugenics [*Volkseugenik*] that considers the whole population of the earth." Once race hatred had been deprived of its necessary base in the capitalist order, Levien asserted, man "will become a creative shaper of his own species; this advancement will bring about an upward surge in mankind's intellectual achievements and the power of science will thus enhance the rational, practical use of the laws of genetics for the development of an undreamt-of higher kind of man."[27] Levien's Communist eugenics was to be a part of an overall *Volkshygiene* program that included measures for both genetic and environmental improvement.

Levien's argument was of especial relevance to the Soviet Union. There the successful revolution he dreamed about had already occurred. The journal in which he wrote was the Western Communist theoretical publication most widely circulated among Marxist intellectuals in the Soviet Union. Indeed, many of the articles were written by Soviet citizens. Would Soviet Marxists, freed of the capitalist order, follow Levien's suggestion by developing a socialist eugenics?

OBSERVERS OF ITS EARLY HISTORY are frequently surprised to learn that Soviet Russia in the 1920s possessed a strong eugenics movement. One might have expected revolutionary Russia, which prided itself on opposition to capitalist culture and aristocratic privilege, to have stood aside from the movement for "race betterment" which swept the world in those years and led to the establishment of eugenics societies in dozens of countries. To arrive at this conclusion, however, is to carry back into the third decade of this century ideas both about eugenics and Soviet views of man which took clear form only in later years. Here, as in Germany, there was a gradual formation of value links to scientific ideas, only in a very different political setting.

Scholars who have investigated the history of Darwinism in Russia in the late nineteenth century have noted the unusual enthusiasm with which it was received by intellectuals there. One historian wrote, "Unlike its reception in the West, Darwinism met almost no opposition in Russia from either the scientists or the social thinkers."[28] One reason for this receptivity was that by the last decades of the empire the intelligentsia of Russia usually defined itself

[26] Levien, "Stimmen aus dem deutschen Urwalde (Zwei neue Apostel des Rassenhasses)," *Unter dem Banner des Marxismus*, 2 (1928): 150–95. The article is a criticism of the views of H. F. K. Günther and A. Basler.

[27] *Ibid.*, 155, 163.

[28] James Allen Rogers, "Charles Darwin and Russian Scientists," *The Russian Review*, 19 (1960): 382. There was even an early advocate in Russia of improvement of humans through selection: V. M. Florinskii,

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in oppositional terms to the Church and monarchy; intellectuals seized upon scientific explanations of man's origins as natural allies in their struggle against these authorities. In addition, the intelligentsia wanted to close the gap between the culture of Russia and that of Western Europe; in their effort to do so, they often embraced rather uncritically the latest scientific trends coming from the West.

By the first decades of the twentieth century, however, biologists in Russia had moved far beyond mere imitation of Western science and had formed a center of outstanding genetics research, entirely in step with the new trends and in some respects even leading the way. Around the figure of S. S. Chetverikov a school of population genetics was established which is not fully appreciated even yet by historians of biology, largely because it disappeared in later years when Lysenko took over Soviet genetics.[29] In addition to Chetverikov, prominent early Soviet biologists included N. K. Kol'tsov, A. S. Serebrovskii, N. I. Vavilov, and Iu. A. Filipchenko. Several of these distinguished scientists were also heavily involved in the eugenics movement.[30] One of the outstanding younger Russian biologists was Theodosius Dobzhansky, who later emigrated to the United States, where he became world-famous. Dobzhansky's career began with a position in the Bureau of Eugenics of the Soviet Academy of Sciences.

The Russian eugenics movement was limited almost entirely to the years 1921–30, and this period can itself be divided into two phases, with the division around 1925. In the first phase, Russian eugenics developed along lines quite similar to the movement in a number of other countries. The German example was probably the most significant, largely because of the number of German monographs on the subject and the closeness of Russian and German scientific relations. In the second phase, after 1925, Soviet eugenists made an effort to create a unique socialist eugenics of their own, an effort which met increasing opposition. Lysenko and the form of Stalinist genetics which later became notorious throughout the world were not involved in either of these phases (these developments came only in the late 1930s and 1940s); nonetheless, the

Usovershenstvovanie i vyrozhdenie chelovecheskogo roda (St. Petersburg, 1866). For a discussion of this book by an early Soviet eugenist, see M. V. Volotskoi, "K istorii evgenicheskogo dvizheniia," *Russkii evgenicheskii zhurnal*, 2 (1924): 50–55. Also see George Kline, "Darwinism and the Russian Orthodox Church," in Ernest J. Simmons, ed., *Continuity and Change in Russian and Soviet Thought* (Cambridge, Mass., 1955), 307–28; N. Danilevskii, *Darvinizm, kriticheskoe issledovanie* (St. Petersburg, 1889); K. A. Timiriazev, *Charlz Darvin i ego uchenie* (3d ed.; Moscow, 1894); and Alexander Vucinich, "Russia: Biological Sciences," and James Allen Rogers, "Russia: Social Sciences," in *The Comparative Reception of Darwinism*, 227–68.

[29] See Mark B. Adams, "The Founding of Population Genetics: Contributions of the Chetverikov School, 1924–1934," *Journal of the History of Biology*, 1 (1968): 23–39, and "Towards a Synthesis: Population Concepts in Russian Biological Thought, 1925–1935," *Journal of the History of Biology*, 3 (1970): 107–29.

[30] N. K. Kol'tsov was president of the Russian Eugenics Society; Iu. A. Filipchenko was the director of the Bureau of Eugenics of the Academy of Sciences; and A. S. Serebrovskii was a member of the permanent bureau of the Russian Eugenics Society and a contributor to its journal. For his great hopes for the eugenics movement, see A. S. Serebrovskii, "O zadachakh i putiakh antropogenetiki," *Russkii evgenicheskii zhurnal*, 2 (1923): 107–16, esp. 112. Ironically, N. I. Vavilov, who ultimately suffered the most at the hands of Soviet authorities, apparently steered clear of eugenics; he became a foe of Lysenko in the late 1930s and died in 1940 in Siberian exile.

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controversies over eugenics in the 1920s were important elements in the foundation of later Soviet attitudes toward genetics.[31]

The two most important organizations in the Soviet eugenics movement were the Russian Eugenics Society and the Bureau of Eugenics of the Academy of Sciences, both created in 1921.[32] Each of them published a journal, the first entitled the *Russian Eugenics Journal,* the second (in its initial form) the *Bulletin of the Bureau of Eugenics*. The latter was more academic than the former and also proved to be considerably more sensitive to the political opposition that soon developed to the eugenics movement. The members of the Bureau of Eugenics were professional geneticists, interested in studying human heredity from a scientific standpoint and anxious to protect this study from political considerations, while the members of the Russian Eugenics Society were a heterogeneous collection of scholars and lay people. The popular aspirations of the society were illustrated by its call at the end of its first year

> for cooperative work on eugenics by scientific and social workers in all specialties. As an expression of the most vital cultural problems, as a question of the eventual fate of culture, the eugenic idea must sooner or later gather under its banner all intellectually aware human beings. We must hurry, while there is still time, while it is still possible to ward off many of the misfortunes that are threatening cultured society, misfortunes that are the result of the one-sided development of modern civilization.[33]

The historian who today leafs through the pages of the *Russian Eugenics Journal*, knowing well the class antagonisms and radical currents still waiting to be expressed in Soviet society, is struck by the naiveté and blindness to political complications of the early leaders of the movement. One of the early concerns of the journal's authors in the years immediately after the Revolution was the genealogy of outstanding and aristocratic Russian families; investigations, complete with family tables, were made of princely families of exemplary achievements, as well as of all the members of the Academy of Sciences in the previous century. Several writers expressed dismay about the dysgenic effects of the Russian Revolution. The emigration of the nobility and of other upper-class families as a result of the Revolution was seen as a serious loss to the genetic reserves of Russia, requiring eugenic correction. More predictable, perhaps, was concern about the enormous Russian losses during World War I, far heavier in absolute (though not relative) terms than those of any other nation.[34] One Soviet eugenist, V. V. Bunak, summed up the ef-

[31] For a good bibliography of Russian eugenics literature, see K. Gurvich, "Ukazatel' literatury po voprosam evgeniki, nasledstvennosti i selektsii i sopredel'nykh oblastei, opublikovannoi na russkom iazike do I/1 1928 g.," *Russkii evgenicheskii zhurnal*, 6 (1928): 121–43. For a discussion of the eugenics movement in Soviet Russia, see David Joravsky, *The Lysenko Affair* (Cambridge, Mass., 1970), 256–66.

[32] For accounts of the founding and early activities of the Russian Eugenics Society, see "O deiatel'nosti Russkogo Evgenicheskogo Obshchestva za 1921 god," *Russkii evgenicheskii zhurnal,* 1 (1922): 99–101, and similar descriptions in succeeding volumes of the same publication. For an interesting but somewhat one-sided recent Soviet interpretation of these early eugenic interests, see N. P. Dubinin, *Vechnoe dvizhenie* (Moscow, 1973).

[33] "O deiatel'nosti Russkogo Evgenicheskogo Obshchestva za 1921 god," 101.

[34] For examples of these types of literature, see A. S. Serebrovskii, "Genealogiia roda Aksakovykh," *Russkii evgenicheskii zhurnal*, 1 (1922): 75–82; N. Chulkov, "Rod grafov Tolstykh," *Russkii evgenicheskii zhurnal,*

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fects of eight years of war, revolution, civil war, and famine in the statement that the total genetic impact "might exceed that of the West" in the same period. Thus, the deep eugenic gloom of postwar Germany was reflected among Russian eugenists, although there was some hope for backward Russia in that "the more cultured a country, the more the biological danger of war."[35]

The favored word in Soviet Russia for the subject was "*evgenika*" (eugenics), although the term "*rasovaia gigiena*" (race hygiene) was also used and racial interests were widespread. The Russian Eugenics Society soon established a special commission for the study of the "Jewish Race," following a major interest of the German movement.[36] In one study the commission concluded that Jews were in no way inferior to other ethnic groups. Despite the conclusion, the Russian eugenics movement explicitly thought in terms of racial differences—psychological as well as physiological—and followed the Germans in accepting such terms as the biologically meaningless "Jewish race." The *Russian Eugenics Journal* frequently contained reviews of German books that were establishing the guidelines of race hygiene in terms of a static or typological definition of race (such as the famous textbook on human heredity by Baur, Fischer, and Lenz), and the reviewers were usually impressed by the new doctrines.[37] The first issue of the Russian journal contained reviews of fourteen German books on human heredity and eugenics and no others. Kol'tsov and Filipchenko also wrote several articles for the main German eugenics journal, using the terms "*Die rassenhygienische Bewegung*" and "*Die rassenhygienische Literatur*" to describe Russian trends.[38]

3 (1923): Iu. A. Nelidov, "O potomstve barone Petra Pavlovicha Shafirova," *Russkii evgenicheskii zhurnal*, 3 (1925): 61–66; V. Zolotarev, "Rodoslovnye A. S. Pushkina, gr. L. N. Tolstogo, P. Ia. Chaadaeva, Iu. F. Samarina, A. I. Gertsena, kn. P. A. Kropotkina, kn. S. N. Trubetskogo," *Russkii evgenicheskii zhurnal*, 5 (1927); S. V. Liubimov, "Predki grafa S. Iu. Vitte," *Russkii evgenicheskii zhurnal*, 6 (1928): 203–13; Iu. A. Filipchenko, "Nashi vydaiushchiesia uchenye," *Izvestiia buro po evgenike*, no. 1 (1921); T. K. Lepin, Ia. Ia. Luss, and Iu. A. Filipchenko, "Deisvitel'nye chleny byvsh. imperat., nyne Rossiiskoi, Akademii za poslednie 80 l. (1864–1924 gg.)," *Izvestiia buro po evgenike*, 3 (1925); L. S. Berg, "Brachnost' rozhdaemost' i smertnost' v Leningrade za poslednie gody," *Priroda*, nos. 7–12 (1924); A. V. Gorbunov, "Vliianie mirovoi voiny na dvizhenie naseleniia Evropy," *Russkii evgenicheskii zhurnal*, 1 (1922): 40–64; and V. V. Bunak, "Novye dannye k voprosu o voine, kak biologicheskom faktore," *Russkii evgenicheskii zhurnal*, 2 (1923): 223–32.

[35] Bunak, "Novye dannye," 231.

[36] The chairman of the "Commission for the Study of the Jewish People" was V. V. Bunak. "Evgenicheskie zametki," *Russkii evgenicheskii zhurnal*, 2 (1924): 58. For examples of the commission's work, see S. S. Vermel', "Prestupnost' evreev," *Russkii evgenicheskii zhurnal*, 2 (1924): 153–58; and V. V. Bunak, "Materialy dlia sravnitel'noi kharakteristiki sanitarnoi konstitutsii evreev," *Russkii evgenicheskii zhurnal*, 2 (1924): 142–52.

[37] See, for example, Kol'tsov's generally positive review of E. Baur, E. Fisher, and F. Lenz, *Grundriss der menschliche Erblichkeitslehre und Rassenhygiene*, in *Russkii evgenicheskii zhurnal*, 2 (1924): 179–80. Kol'tsov commented that Lenz's section of this textbook had been an important source for his own article, "Vliianie kul'tury na otbor v chelovechestve," *Russkii evgenicheskii zhurnal*, 2 (1924): 3–19. Filipchenko chose as a part of the title of one of his articles a quotation from the same book, Lenz's observation that "der Schutz der geistigen Arbeiter, und speziell der hochbegabten, ist eine Hauptaufgabe der Rassenhygiene." Iu. A. Filipchenko, "Nashi vydaiushchiesia uchenye," 22.

[38] See, for example, Kol'tsov's "Die rassenhygienische Bewegung in Russland," *Archiv für Rassen- und Gesellschaftsbiologie*, 17 (1925): 96–99; and Filipchenko's "Die russische rassenhygienische Literatur, 1921–1925," *Archiv für Rassen- und Gesellschaftsbiologie*, 3 (1925): 346–48. The German race hygienists paid particular attention to research done by Soviet scholars in the Ukraine that purportedly indicated that the cranial measurements of intellectuals are greater than those of workers and peasants—this in a "workers' and peasants'" state! See "Materialien zur Anthropologie der Ukraine," *Archiv für Rassen- und Gesellschaftsbiologie*, 25 (1931): 340–42.

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The leaders of the Russian eugenics movement were anxious for contacts with other national eugenics societies and, indeed, worked for full international recognition. The fact that Soviet Russia was a revolutionary state that attacked the practices and world-views of leading nations in most areas of international political and cultural relations seems not to have deterred the early activities of Soviet eugenists. They established contacts with the Eugenic Education Society in England, the Eugenic Record Office in the United States, and the German Society for Race and Social Biology. In November 1921 the Russian Eugenics Society was recognized as a full member of the International Eugenics Union with headquarters in London. But in the international arena, Bolshevik Russia—as well as the former enemy nation Germany—was ostracized by the former Allies at many international scholarly congresses after World War I, and this policy extended to eugenics, despite the formal contacts. Thus, neither Soviet Russia nor Germany was invited to the Second International Congress of Eugenics in New York in 1921, a slight that deeply rankled Russian eugenists.[39] This ostracism, however, had the effect of deepening scholarly connections between the two pariah nations. The president of the Russian Eugenics Society, the biologist N. K. Kol'tsov, did manage to obtain an invitation to the Third International Congress of Eugenics in Milan in 1924, where he lamented that the influence of the Catholic Church was so prevalent that the participants were one-sidedly cautious in their discussions of practical eugenic measures. He also noted that, if the attitudes of Catholic Italians were one extreme, the Americans, with their system of sterilization laws, were another. No Americans or Germans were present at the Congress, and, according to Kol'tsov, not one participant spoke in favor of the American system of sterilization.[40]

The eagerness of Russian eugenists for international contacts and recognition was also evident in the pages of the *Russian Eugenics Journal*, which regularly printed news of the other societies as well as summaries of the publications of the other eugenics journals; the German *Archive for Race and Social Biology* received the greatest attention.[41] When the German Society for Race Hygiene passed a series of resolutions in 1922 calling for greater attention by legislators to eugenics, the *Russian Eugenics Journal* printed the resolutions in full without comment.[42] Similar attention was given to proposals of the English and Swedish eugenics societies.[43] In 1924 the journal gave equal play to a German physician who appealed to all of his "colleague-physicians both in the cities and in the countryside to search for defective persons, taking

[39] For his irritation, see N. K. Kol'tsov, "Kritika i bibliografiia: Trudy 2-go mezhdunarodnogo evgenicheskogo s"ezda," *Russkii evgenicheskii zhurnal*, 2 (1924): 69.

[40] Kol'tsov, "Evgenicheskie s"ezdy v Milane v Sentiabre 1924 goda," *Russkii evgenicheskii zhurnal*, 3 (1925): 73–77.

[41] For a contemporary survey of the international eugenics movement, describing the various legislative programs, see P. I. Liublinskii, "Sovremennoe sostoianie evgenicheskogo dvizheniia," *Russkii evgenicheskii zhurnal*, 4 (1926): 53–75.

[42] "Rukovodiashchie polozheniia nemetskogo obshchestva rasovoi gigieny," *Russkii evgenicheskii zhurnal*, 1 (1923): 364–66.

[43] "Sovremennoe sostoianie voprosa o sterilizatsii v Shvetsii," *Russkii evgenicheskii zhurnal*, 3 (1925): 78–80; and "Programma prakticheskoi evgenicheskoi politiki," *Russkii evgenicheskii zhurnal*, 5 (1927): 37–40.

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advantage of the help of local authorities, teachers, philanthropic institutions, medical personnel, and to sterilize as many of them as possible."[44] Although the Russian eugenists shared many assumptions of the international eugenics movement in general and of the German movement in particular, they should not be seen as proto-National Socialists. Substantive links between the Russian eugenics movement and incipient fascism were completely lacking. Similar eugenics organizations existed in most Western nations at that time and, in fact, were far stronger in England and the United States than they were in Russia.

Several early Soviet eugenists showed awareness that their analysis and program were potentially inflammatory from a political standpoint. In the early spring of 1925 the Russian Eugenics Society debated eugenics proposals that had been advanced in other countries, and some Soviet participants objected to the "coarseness" and ill-defined quality of a number of these proposals. The Leningrad branch of the society dissociated itself from plans to prohibit racially mixed marriages, and they voted in favor of sterilization "only on the basis of a decision of a special council of competent individuals and with the permission of the individual or his legal representatives and only on eugenic, not social, grounds." They did, however, vote in favor of obligatory isolation from the opposite sex of "certain categories of the mentally ill and habitual criminals whose reproduction would be dangerous for society on social and eugenic grounds."[45] But the real significance of the Russian eugenics movement lay not in its relationship to Western eugenic proposals, interesting as those connections were, but in its place in the debates over the nature of man that were beginning to take place in Soviet Russia itself. How was the eugenics movement perceived by Soviet intellectuals outside the community of eugenists? What was the relation of the doctrines of eugenics to the ideas of Russian socialism and Communism?

The great debate over the issue was slow in developing. The commissar of public health, Nikolai Semashko, had given his approval to the eugenics movement; the Commissariat of Internal Affairs (a police organization) formally accepted the charter of the Russian Eugenics Society; and the Russian Eugenics Society received a small state subsidy.[46] These first official acts of recognition were probably not too significant in themselves, since in the early years the concept of eugenics was so new to most people in Soviet Russia, the essential issues so unexplored, that a sophisticated understanding of the movement by bureaucrats was hardly possible. To the extent that eugenics

[44] "Evgenicheskaia sterilizatsiia v Germanii," *Russkii evgenicheskii zhurnal*, 3 (1925): 82.

[45] "Obsuzhdenie Norvezhskoi evgenicheskoi programmy na zasedaniiakh Leningradskoi Otdeleniia R. E. O.," *Russkii evgenicheskii zhurnal*, 3 (1925): 139–43. The particular eugenic program at the basis of this discussion was that of the Norwegian J. A. Mjöen. This society's discussion is very interesting, for it encompassed consideration of both "positive and negative race hygiene proposals" by speakers with wide disagreements on issues such as racially mixed marriages and mandatory sterilization.

[46] For approval by the Commissariats of Health and Education, as well as for announcement of the subsidy, see "Iz otcheta o deiatel'nosti Russkogo Evgenicheskogo Obshchestva za 1923 g.," *Russkii evgenicheskii zhurnal*, 2 (1924): 4. For approval of the society's charter by the Commissariat of Internal Affairs, see "Evgenicheskie zametki," *Russkii evgenicheskii zhurnal*, 2 (1924): 58.

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was understood, it was thought to be the science for the collective improvement of mankind, and as such it was an activity that the young Soviet government automatically found interesting. In 1925 the debate that had begun to simmer in lecture halls and in local publications spilled out into major Soviet intellectual journals. The activities of the Russian Eugenics Society and the Bureau of Eugenics—up to this time primarily the concerns of a rather small circle of geneticists, physicians, public health specialists, and university students studying the same fields—now became an object of public attention.

In one of the first comprehensive and critical analyses of the eugenics movement, Vasilii Slepkov's article, "Human Heredity and Selection: On the Theoretical Premises of Eugenics" (which appeared in April 1925 in the major Bolshevik journal of Marxist theory), pointed out that the eugenists were emphasizing biological determinants of human behavior to the total neglect of socioeconomic determinants. Since most eugenists were biologists with little knowledge of the social sciences and especially of Marxism, they had "absolutized" the influence of heredity to the detriment of the influence of the environment and thereby converted genetics into a "universalist" interpretation in which all of human history was merely a story of the replacement of one genotype by another. They had accepted the views of exponents of the Nordic race, who explained European history in terms of the domination by superior genotypes who became the European nobility. This conviction necessarily led to cultural pessimism, since the eugenists believed that the nobility was no longer reproducing at a rate sufficient to insure its survival. According to Slepkov, this point of view totally ignored the principles of Marxism, which demonstrated that social conditions determine consciousness. Slepkov quoted Friedrich Engels on the importance of labor in the evolution of primates, Georgii Plekhanov's point that conservative thinkers had always explained human behavior on the basis of innate qualities in order to avoid social analysis leading to revolutionary conclusions, and Karl Marx's concept that "people are a product of conditions and education and, consequently, changing people are a product of changing circumstances and different education."[47]

A thief is not a biological type, created by heredity, said Slepkov, but a "social man" created by his environment, poverty, and unemployment. By emphasizing the hereditary differences of individuals to the complete neglect of social influences, the eugenists were inexcusably distorting the general understanding of human nature. Slepkov did not deny, however, that individuals differ genetically. In order to explain those differences he resorted to a position that was to have a long, and eventually tragic, influence on Soviet biology: the inheritance of acquired characteristics. He believed that Pavlov's research had demonstrated the influence of the social environment on human behavior. And he pointed out that Pavlov believed that conditioned reflexes

[47] Slepkov, "Nasledstvennost' i otbor u cheloveka (Po povodu teoreticheskikh predposylok evgeniki)," *Pod znamenem marksizma,* 10–11 (1925): 79–114.

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acquired during the lifetime of an organism could become, in some cases, permanently hereditable. Slepkov also referred to the research of Kammerer on salamanders as additional evidence of the inheritance of acquired characteristics, ignoring the fact that Kammerer's work was rejected by the great majority of biologists. Kammerer's political radicalism, however, was well known; Slepkov was thus able to strengthen his proposed fit of Lamarckism and political reformism.

As a criticism of their naive and one-sided biologism, much of Slepkov's castigation of the eugenists was appropriate; but, by placing so much emphasis on the inheritance of acquired characteristics, Slepkov praised antique biology in order to humanize modern biology. A more judicious criticism would have been to dissect the dubious assumptions of eugenists that society was threatened by a biological degeneration that demanded social and legislative solutions. For many years, however, the inheritance of acquired characteristics remained only as a remote candidate to replace classical genetics. The geneticists and eugenists still had very powerful arguments at their disposal. Even in terms of politics, in the Soviet Union during the mid-1920s few assumed that Marxist socialism and eugenics were incompatible or believed that Marxism and Larmarckism were uniquely compatible.

Not only the eugenists with their arrogant programs for the biological reform of society but also the more sober and scientific geneticists interested primarily in animal and plant heredity were beginning, nevertheless, to meet stiff opposition from radical students in Soviet universities. One biologist with a political commitment to the new regime, B. M. Zavadovskii, wrote that each year, when he gave lectures at the Sverdlov Communist University, the radical students reacted with hostility to his discussion of heredity; they called genetics "a bourgeois science" which contained implications that were unacceptable to the proletariat. Voices in favor of Lamarckism were becoming "louder and stronger," he wrote in 1925, as was the belief that genetics contradicted Marxism and the social policy of the Communist party. "This point of view is receiving support in the psychology of the masses," he commented, "whose first reaction to genetics is negative." Zavadovskii recognized that among educated Marxist scientists and social theorists the attitude toward genetics was much more positive than among students and lay people, since serious Marxists were anxious not to contradict the findings of science and many fully recognized the scientific baselessness of Lamarckism. Yet Lamarckism continued to grow, especially among pedagogues, who believed that education was much more important than heredity. Zavadovskii feared that, in dispensing with the erroneous views of the eugenists, Marxists might "throw out the baby with the bath water" and eliminate the science of genetics as well. Marxists, he said, have been "frightened" by the conclusions of bourgeois eugenists.[48]

By 1925, then, the first crisis in the debate between Mendelian or classical

[48] Zavadovskii, "Darvinizm i lamarkizm i problema nasledovaniia priobretennykh priznakov," *Pod znamenem marksizma*, 10–11 (1925): 79–114.

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genetics and Lamarckism was coming to a head in the Soviet Union, and the eugenists were in the middle of it. The eugenists began to defend both genetics and eugenics simultaneously, and they considered the Lamarckians to be one of the most important groups among their opponents. As early as 1924 the most prominent Russian eugenist, Kol'tsov, had published an article—"New Attempts to Prove the Inheritance of Acquired Characteristics"—in which he had dismissed such efforts as "the replacement of science by faith."[49] Several of the eugenists realized that the criticisms advanced against them were so serious that, unless some way could be found to reconcile their understanding of human heredity with Marxist aspirations, not only was the eugenics movement in danger but genetics as well. Iurii Filipchenko, one of the brightest of the eugenists, found an argument in favor of genetics that carried great weight, and for a time some observers thought he had outmaneuvered the radical critics of genetics. Let us assume for a moment, said Filipchenko, that the Lamarckians are correct and the Mendelians incorrect, even though scientific evidence at the moment all points in favor of the classical Mendelian theory. What would be the result for the proletariat, for the lower classes in general, and for the cause of social revolution if acquired characteristics were inherited? Most people seem to believe that such a theory points to rapid social reform, since by giving individuals a better environment, not only would those individuals benefit during their lifetimes, but supposedly the good effects of that better environment would be passed on to subsequent generations, holding out the possibility of rapid improvements built on a successively improving genetic base. The view of an unchangeable genotype contained in classical genetics, on the other hand, seems to set iron limits to such improvement and therefore seems inherently conservative in its social implications.[50]

This view was superficial and false, said Filipchenko, because it assumed that only "good" environments have hereditable effects, while a consistent interpretation of the inheritance of acquired characteristics would show that "bad" environments also have effects. Therefore, all socially or physically deprived groups, races, and classes of people—such as the proletariat and peasantry and the nonwhite races—would have inherited the debilitating effects of having lived for centuries under deprived conditions. Far from promising rapid social reform, the inheritance of acquired characteristics would mean that the upper classes are not only socially and economically advantaged, but genetically privileged as well, a result of centuries of living in a beneficial environment. Thus the proletariat in Soviet Russia would never be capable of running the state; it was genetically lamed by the inheritance of the effects of its poverty. If the classical geneticists were correct, on the other hand, said Filipchenko, then combinations of genes were distributed throughout the lower classes that would give the individuals possessing them all the

[49] Kol'tsov, "Noveishie popytki dokazat' nasledstvennost' blagopriobretennykh priznakov," *Russkii evgenicheskii zhurnal,* 2 (1924): 159–67, esp. 167.

[50] For a discussion of the impact of Filipchenko's argument, see N. M. Volotskoi, "Spornye voprosy evgeniki," *Vestnik kommunisticheskoi akademii,* 20 (1927): 224–25.

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possibilities for being great scientists, musicians, artists, or whatever they might wish to be, if only the exploitative, debilitating environment were eliminated.

The logic of Filipchenko's argument seems to have caused critics of the geneticists to hesitate. Several radical journals ran articles which maintained that only the inheritance of acquired characteristics, not Mendelian genetics, was counterrevolutionary. One author stated that the international bourgeoisie constantly renewed efforts to establish the inheritance of acquired characteristics in order to show its own genetic superiority, but the proletariat was learning that science spoke against them. Another author, writing in *The Red Journal for All People*, said that every social reformer should read Filipchenko's argument in order to be armed for the political struggle.[51]

For several reasons, Filipchenko's argument was not persuasive enough, however, to stem the popular belief that the inheritance of acquired characteristics was more congenial to the idea of creating a new society and a new culture than was Mendelian genetics. First of all, the majority of radical reformers, in a time of social revolution, either never listened to or never understood Filipchenko's fairly sophisticated argument. Simple arguments won out over subtle ones. Many commentators agreed with Zavadovskii that the first reaction of young Soviet students, on hearing of the dispute between classical genetics and Lamarckism, favored Lamarckism because of its apparent possibilities for the rapid transformation of man. In other countries as well the nature-nurture argument has often been equated, quite exaggeratedly, with a political split between the right and the left. And even at the source there seemed to be some support for this view, for Lamarck had himself risen to high position after the French Revolution. Events in Germany in the late 1920s seemed to confirm the reverse side of the historical analogy: conservative race hygienists followed strict Mendelian genetics. Reasons for hesitation in making this neat equation—that Lamarckism has many conflicting interpretations of which a number are not closely linked to reformism or materialism, that Filipchenko's argument was logically stronger than those of his critics, that the whole question of directly linking politics to biology is intellectually dubious—all of these subtle points faded before that first and simple assumption of lay people that genetics seemed to set absolute limits to human improvement while the inheritance of acquired characteristics seemed to open limitless possibilities.

Even those relatively few scientifically educated Soviet participants in the debate who listened to Filipchenko's view and understood it were not all convinced, some for fairly good reasons. After all, if Filipchenko were correct about the debilitating effects of an assumed inheritance of acquired characteristics on the lower classes, a great deal would depend on how long it would take to erase those effects—many generations or one or two? Furthermore, the most that Filipchenko could promise on the basis of his classical genetics was that *some* members of the proletariat could excel because of their fortu-

[51] *Ibid.*, 225.

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itous possession of the right genes. The eventual result would be a class society based on innate, unchangeable ability, a meritocracy. The inheritance of acquired characteristics held a more radical, democratic prospect: *all* members of the lower classes and their progeny might advance equally because of their social environment, and, given sufficient time, there was no reason for the former lower class to be at any genetic disadvantage to the former upper class.

Several Marxist scholars tried to develop sophisticated arguments to meet Filipchenko's intellectual challenge. Volotskoi, who had abandoned his earlier commitment to eugenics based on classical genetics and moved toward a Lamarckian eugenics, held that Filipchenko's mistake lay in believing that the genetic effects of an assumed inheritance of acquired characteristics would be unilaterally harmful to those who came from a background of exploitation and unilaterally useful to those who came from a privileged background. Marxists understood, said Volotskoi, that the division of labor that led to societies based on slaveowners and slaves, lords and peasants, or capitalists and workers has harmful *and* beneficial effects for both groups. In a host-parasite relationship in botany or biology, the parasite often degenerates because it obtains so much of its subsistence from the host; and the slaveowners, nobles, and capitalists of history have similarly degenerated while benefitting from the lower classes. On each side of exploitative relations, said Volotskoi, there is a balance sheet of plusses and minuses: the proletariat suffers from poverty, lack of culture, and unsanitary conditions, but it benefits from the physical labor and hard work necessary for survival; the capitalists benefit economically from their privileged position, but they do not have to labor and become slothful and corrupt. Therefore, Volotskoi continued, Filipchenko was incorrect in believing that an assumed inheritance of acquired characteristics would work only to the benefit of the upper classes; its effect would be mixed. The solution, he maintained, was to abolish the division of labor and conditions of privilege and suffering, for then human beings could be perfected through better social environment.[52]

Yet, in trying to refute Filipchenko, Volotskoi enlarged an assumption that was increasingly criticized by Soviet Marxist theoreticians: the belief that biological laws could be easily extended to human beings. It would be better, the theoreticians maintained, to divide human and animal or plant heredity and to recognize the nonreductive and qualitatively distinct nature of man. In that way many difficult problems could be avoided. Furthermore, dialectical materialism, the Soviet Marxist philosophy of nature, incorporated this theory of nonreductive levels into its overall perspective. Volotskoi confirmed the suspicions of the Marxist theoreticians by marching right ahead with the idea of eugenics, much as he decried "bourgeois eugenists."[53] Indeed, in his

[52] *Ibid.*, 212–54.

[53] For an example of the type of extrapolation from the animal and plant worlds to that of human society which increasingly attracted criticism, note Filipchenko's observation: "Every plant and animal breeder who graduates from an agronomy institute is familiar in detail with the genetics of domestic animals and

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promotion of a socialist eugenics based on Lamarckian biology, he went so far as to suggest mandatory sterilization and thus fell into the position of a small minority.

These debates essentially resulted in a draw among scientists on the issue of whether or not the inheritance of acquired characteristics was theoretically more consonant with Marxism than was classical genetics. On a popular level, however, the belief among lay people and students that genetics was a bourgeois science continued to be strong and even grew as social conflict increased at the end of the decade with the commencement of the five-year plans. The obvious losers in the debate were the Russian eugenists who defined their field in terms of the Western eugenics movement. They were not only supporters of classical genetics, with all of the controversy that field attracted in the Soviet Union, but they went far beyond these theoretical principles to extrapolations of biology to society. In these extrapolations they included a host of assumptions about the future of society, the nature of races and classes, and the relative influence of nature and nurture that did, indeed, conflict with prevalent views among the politically active elements of Soviet society. As a result of their possession of these distinct and controversial views and of their links to ideologically hostile foreign scholarship, the Russian eugenists became more and more easily defined as "bourgeois specialists," an appellation that increasingly became akin to a criminal charge.

The eugenists recognized that they must either abandon their concerns or radically change their activities in order to demonstrate that they actually had the interests of Soviet socialism, not Western capitalism, at heart. The scientists in the Bureau of Eugenics at the Academy of Sciences chose the prudent path of abandoning the field. Between 1925 and 1928 they shifted from a concern with human heredity to a concern with the genetics of plants and animals. The change of emphasis can easily be seen in the successive titles of their leading journal:

1922–1925	*Bulletin of the Bureau of Eugenics*
1925–1927	*Bulletin of the Bureau of Genetics and Eugenics*
1928–	*Bulletin of the Bureau of Genetics*

The last articles predominantly concerned with human genetics appeared in this journal in 1925. Eugenists in the Academy of Sciences became plant and animal geneticists primarily interested in the most exciting experiments in the field of their time, the study of *Drosophila melanogaster*, the fruit fly. But their eugenics past was to be held against them in the troublesome 1930s.

The Russian Eugenics Society chose a more heroic and foolhardy path. The editors of the journal decided to change their emphases, to show how eugenics could be fitted to the purposes of social revolution and Marxism. Turning away from their genealogical studies of the nobility, they began to make stud-

plants; can we really say that the latter are more valuable in these regards than man himself?" Filipchenko then went on to call for the inclusion of eugenics instruction in the standard education curriculum for Soviet youth. See his "Evgenika v shkole," *Russkii evgenicheskii zhurnal*, 3 (1925): 32.

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ies of the reformers and revolutionaries of Russian history, beginning with the Decembrists of 1825 but continuing right up to contemporary Communist party leaders, whom they noted were not reproducing at an adequate rate.[54] Kol'tsov did a study of the genetic sources of the talented young proletarians being promoted up through the ranks of the universities and institutions of the Soviet Union.[55] Other eugenists began developing extended justifications for a unification of the goals of Soviet socialism and eugenics. But the whole effort became increasingly artificial and strained.[56] The eugenists with their record of connections with the international movement—including German race hygiene with its ever-clearer links to National Socialism—could never justify themselves by trying to be more radical than their critics—or even equally radical—much as they were by then attempting. A scholar like Filipchenko could win individual debates through the strength of his arguments about the social implications of an assumed inheritance of acquired characteristics, but by style and background he was always a middle-class *intelligent* to his critics.

By 1930 the eugenics movement in the Soviet Union was finished. These were years in which political controls in the Soviet Union were imposed on many scholarly institutions, and collectivization was violently enforced in the countryside. The Russian Eugenics Society was closed and its publications suspended. By 1931, when the *Large Soviet Encyclopedia* published the volume of its first edition with an article on "Eugenics," the field was simply condemned as a "bourgeois doctrine."[57] All efforts to create a unique, socialist eugenics were ridiculed. By this moment in Soviet history the logical possibility of linking the Marxist desire to transform man with the eugenic desire to improve him had irretrievably disappeared.

The end of eugenics meant the end of discussions of human heredity, but it did not mean immediate victory for the doctrine of the inheritance of acquired characteristics. Debates on that issue had been indecisive, and the Communist philosophers and political leaders did not wish to fall unwittingly into contradiction with science. The terms of the de facto intellectual truce were pragmatic: let the partisans of classical genetics and of the inheritance of

[54] Kol'tsov observed in a 1924 article, "If we calculate the average number of children in the family of each member of the Russian Communist party, that number will no doubt be far from what Gruber cites as necessary for a population group to preserve itself in the overall population. What would we say about a stock-breeder who every year castrated his most valuable producers, not permitting them to multiply? But in cultured society approximately the same thing is occurring before our eyes!" Kol'tsov, "Vliianie kul'tury na otbor v chelovechestve," *Russkii evgenicheskii zhurnal*, 2 (1924): 15. The reference is to Max Gruber, *Ursache und Bekämpfung des Geburtenrückgange im deutschen Reiche* (Munich, 1914).

[55] Kol'tsov, "Rodoslovyne nashikh vydvizhentsev," *Russkii evgenicheskii zhurnal*, 4 (1926): 103–43. For other examples of this type of article, see N. P. Chulkov, "Genealogiia dekabristov Murav'evykh," *Russkii evgenicheskii zhurnal*, 5 (1927): 3–20; P. F. Rokitskii, "Bakuniny," *Russkii evgenicheskii zhurnal*, 5 (1927): 21–24; and V. Zolotarev, "Dekabristy," *Russkii evgenicheskii zhurnal*, 6 (1928): 178–97.

[56] The difficulty of the position of the academic eugenists was illustrated by A. S. Serebrovskii when he called for a Socialist eugenics and then observed, "Every class must create its own eugenics. This slogan, however, . . . must in no way be understood in the manner of several of our comrades, especially in Moscow, who maintain that the whole base of Morganist-Mendelian theory is an invention of the Western bourgeoisie and that the proletariat, creating its own eugenics, must base itself on Lamarckism." Serebrovskii, "Teoriia nasledstvennosti Morgana i Mendelia i marksisty," *Pod znamenem marksizma*, 3 (1926): 113.

[57] "Evgenika," *Bol'shaia Sovetskaia Entsiklopediia*, 23 (1931): cols. 812–19.

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acquired characteristics resolve the issue in practice, in the applied science of agronomy—not in the dangerous field of human heredity where the Germans in the 1930s were winning such notoriety, but in agriculture where the Soviet Union needed help. This truce set the stage for the rise of a clever schemer who could capitalize on the inability of geneticists in the early 1930s to produce agronomic miracles because of the immaturity of their science (hybrid corn and similar genetic innovations were applications of the 1940s), while he, surrounded by political supporters, exposed the eugenic past of a number of the prominent geneticists who tried to show the fallacies of his own innovations. Political loyalties, historical associations, and dedication to practical support of Soviet agricultural policies became far more important in determining the results of the discussions than logical rigor or scientific validity.

By the early 1930s the process of the gradual crystallization of value links to conflicting concepts of heredity—links that were at first by no means clear or inevitable—was far advanced in Germany and Russia. Eventually the two societies went in opposite directions in their interpretations of the nature-nurture controversy. The genetic doctrines and practices of Nazi Germany postulated the existence of genetic differences between races and other categories of man that have much greater importance in determining the worth and performance of individual members of those groups than anything that man could do during his lifetime. Thus, according to this assumption, "nature" obviously far outweighs "nurture." In the same years in which these doctrines became widespread in Germany, an opposite—and equally unsubstantiated—theory of genetics gained support and, finally, official approval in the Soviet Union. Lysenko's theory of agronomy, based on the inheritance of acquired characteristics, assigned an overwhelming weight to the nurture side of the equation. His naively optimistic theory of inheritance granted a much greater degree of control than the results of research in biology had indicated was possible.

These two chronologically simultaneous episodes with contrasting results are probably the best large-scale "test cases" for the question of whether or not different values are inherent in different theories of heredity that we are going to find in the flux of history. The known scientific facts of biology and the tested, verified theories of heredity were the same in both countries. Yet each, on the basis of different value systems, embraced different theories of heredity, and each labored mightily in later years to show the harmony between its biological science and its social morality. What do these examples tell us about the relationship between science and sociopolitical values?

Granted, these two episodes are not ultimately the ideal types for asking such a question, since the sets of variables in the two countries were—not surprisingly—different. For example, the official ideologies which later developed out of the controversies of the 1920s described here were not true

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opposites—that is, they were not mirror images of each other. For that to be the case, two different theories of *human* heredity would have had to be fitted to two different political ideologies. That logical circle was never quite closed, since Lysenko's theory of inheritance was never explicitly applied to man, only to plants and animals. The concept of a Soviet eugenics—of Mendelian, Lamarckian, or whatever kind—was condemned by Russian political leaders after the early 1930s. In a sense, however, that very condemnation is the exception that proves the importance of the interaction of scientific theories and political values. The Germans in the 1930s used eugenics in such a horrifying way that the entire concept of eugenics was discredited even in those countries where both the prevailing value goals and the particular theory of heredity contrasted sharply with the goals and theory of the Germans. The world thus never witnessed the ironic and bizarre scene of two different societies aiming toward two different sets of value goals by applying two different (both incorrect) theories of human heredity. But the potential for this scenario was present.

From the perspective of the present, it may appear that there is a natural alliance between eugenics and conservative, even fascist, sentiments. That link was not logically preordained, however, and was not perceived in the early 1920s by large numbers of radical critics of society. Marxists and socialists of many types then supported eugenics, as did liberals, progressives, and conservatives. In the early 1920s scientists and publicists who were interested in eugenics covered a rather wide range of political sentiment in Weimar Germany and Soviet Russia. In these early years eugenics was a faddish doctrine that was often considered progressive, the latest application of science for the benefit of man. If sometimes supported by aristocratic devotees of genealogical tables or middle-class members of social clubs, it also on occasion had support from committed socialists who believed that cultivation of true talent, rather than mere economic privilege, would destroy class society as it had been known. Both Weimar Germany and Soviet Russia were revolutionary states that had replaced recent monarchies. In the 1920s both were experiencing eclectic periods when the full political implications of the latest scientific hypotheses had still not been formulated.

Were the passing of this early heterodox period and the emergence of a high degree of consensus about the correlation between theories of heredity and political world-views phenomena that touched science itself, or were they only epiphenomena of social and political turmoil? Do different theories of heredity intrinsically contain different value implications? The answers to these questions depend in part on what the main rival theories may be. If the contending theories are defined in terms of their post-1933 vulgarized and absolutized forms (that is, a National Socialist theory categorizing humans according to their "overall" genetic value—with Jews, gypsies, and certain types of "social misfits" at the bottom of the heap—opposed to Soviet theory that dogmatically attributed genetic progress to improvements in man's social environment), then the scientific "theories" in themselves contained by definition

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clear value components. Obviously these absolutized explanations, encased in official ideology and supported by the respective governments, can hardly be classified as "science." Yet this fact should not be used as a convenient exit from the dilemma of the relationship of science to values. This study has intentionally been restricted to the period before 1930, when professional geneticists and eugenists in both Germany and Russia usually defined the contrasting theories in more academic terms. They often saw the difference between vulgarized extrapolations and core theories. Even Fritz Lenz spoke out in the 1920s against National Socialist "excesses" which, he believed, harmed the eugenics movement.[58] He accepted the separation of factual and value statements as an ideal, even though he clearly violated it.

During the German Weimar and Soviet New Economic Policy (NEP) periods, the rival theories, stated in their scientific forms, did not explicitly contain value statements. The rival theories were those of Mendelian genetics and Lamarckism, viewed as scientific alternatives. And to many people they seemed to contain strikingly different value implications. One reason for different value connotations stems from the state of genetics in the 1920s as a pure and an applied science. Whether or not science *qua* science contains values, most agree that technology does have value impacts. Therefore, the significance for values of a particular science will vary as the associated technology for that science develops. One interesting aspect of the development of human genetics in the 1920s is that not only was human heredity understood in an exceedingly inexact fashion, allowing much room for speculation based on social and class motives, but there was no technology available for controlling the genetic constitution of organisms except by selection on the basis of phenotypes. Such selection was offensive to existing values when applied to people, and therefore humane individuals wished for an alternative even if they accepted the goal of more control over human genetics.

The relatively few geneticists who did not fall prey to the political ideologies of the time were left with a very undramatic argument, which could not carry their audiences in times of great social stress when the politically active were striving for answers to societal problems. The rigorous geneticists were saying, in effect: Yes, the science of genetics is in principle applicable to man. Genetics is, however, so immature as a science that any effort to apply it to man now would have disastrous and unpredictable effects. Therefore, do not try to apply human genetics as a science, although continue to believe us when we tell you that science is ultimately the best hope for man.

In revolutionary situations popular audiences are not likely to find such an argument persuasive. Both Germany and Russia were undergoing social and political upheavals, and the audiences turned toward more radical, although

[58] For examples of Lenz's criticisms, see his reviews of L. F. Clauss, *Die nordische Seele*, and of Kurt Gerlach, *Fernstenliebe*, in *Archiv für Rassen- und Gesellschaftsbiologie*, 6 (1925): 445–47 and 330. Also see his more reserved criticism of the notorious journalistic race anthropologist H. F. K. Günther in "Notizen: Günthers Berufung nach Jena." *Archiv für Rassen- und Gesellschaftsbiologie*, 23 (1931): 337–39. Lenz criticized in particular the effort of National Socialist race propagandists to make a mythic, irrational ideal of the Nordic spirit. Lenz believed that racial ideas should be based on science.

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quite different, answers to the problem of heredity, answers that contained possibilities for application. The German academic eugenists (as opposed to the race hygiene anthropologists) like Fritz Lenz were closer to science (as it then existed) when they attacked Lamarckists for their sentimental biology than were the Soviet Marxists who rejected genetics along with eugenics. The tragedy is that those scientists who—contrary to Lenz—remained loyal to genetics as a scientific theory, while rejecting the growing inhumane and antirational extrapolations of German race hygiene, were unable to find appreciative audiences in either Germany or Russia. The reason for their failure no doubt lies largely in the social, political, and economic strains both societies underwent. To give the details of those strains would require a long digression into the social and political histories of Weimar Germany and Soviet Russia. But the existence of these strains was only a necessary, not sufficient, condition for the emergence of radically different attitudes toward heredity. Also involved was a science of heredity so immature that it allowed room for wide speculation while offering few applications that were both reliable and ethical.

Particularly worthy of notice is the argument of the young Alfred Ploetz, still in his humanistic phase, because he emphasized that, if the time ever came when selection could be shoved back before birth, it might be possible to develop eugenic measures that were less offensive to traditional values than many of the proposals advanced in his day. Today, Ploetz's observation has more meaning—at least for specific genetic defects—than it did when he wrote. The differential impact on values of various technologies thus serves as a further warning that we should not attribute values easily to scientific knowledge itself; the more important value-determinant is likely to be the technology derived from that knowledge.

We are now approaching the core issue in the question of the relationship of science to values as revealed in our study of Germany and Russia in the 1920s. We have already separated the question of political and social motivations from those of scientific inquiry to the maximum degree that is possible (it is never entirely possible) by focusing on the more academic debates of the 1920s, not the ideological exhortations of the 1930s. We have separately considered the question of technology, trying to show where it had differential impact on value that is distinguishable from that of scientific theory. As the layers of this problem are peeled away, the central question—one that relates to scientific theory alone—emerges: Was there something inherent in a hypothetical Lamarckian theory of heredity that made it a substantial buttress for egalitarian political values, and something within the Mendelian theory of heredity that lent support to elitist views of society? Iurii Filipchenko tried to come to grips with this problem in 1926 when he maintained, with some success, that a hypothetical Lamarckian theory of heredity, followed to its conclusions, would mean the genetic impairment of those social classes whom left-wing social reformers wanted to help. The existence for many generations of the deprived classes in poor environmental conditions would supposedly

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have resulted in the acquisition of hereditary characteristics that would genetically impair them.

This argument was also used by the American geneticist and socialist Hermann J. Muller in his criticism of Lamarckism. (Muller and Filipchenko knew each other and the connection may not be coincidental.) Yet the argument by itself means very little. First of all, it is not completely clear that in a hypothetical case deprived environments would lead uniquely to genetic damage; some people might maintain the reverse, seeing a sharp selection of the "fittest." Since Lamarckism is a doctrine without empirical support, there is no way of knowing how a hypothetical Lamarckism would fit with normal Darwinian selection. Furthermore, no one knows—nor can know in the absence of facts or verifiable hypotheses—how many generations would be required for a hypothetical Lamarckism to overcome the effects of earlier genetic impairment—one generation or many?

Two assumptions were carried by most Lamarckists into the debates over human heredity in the Soviet Union of the 1920s: either deprived environments do *not* result in impaired heredity, or, if deprived environments *do* result in impaired heredity, this impairment can be easily erased in a generation or two in the future through Lamarckian inheritance in improved conditions. If one makes these assumptions, then Lamarckian views truly fitted more comfortably with social reformism than Mendelian genetics, and this "science" contains real value implications. The Lamarckists could then promise the quick advent of a bright genetic future for all members of the proletariat (or any other group) while the Mendelians could promise such a future only to the fortunate few. This formulation of the argument was the generally accepted version in the Soviet Union by the 1930s. Filipchenko lost the debate, the Lamarckists eventually won. But it should be clear that the chains of assumptions here took both into unreal worlds.

On examining the relative value ingredients supposedly inherent in different theories of heredity, we see that Filipchenko was not the only person to maintain that positive, humane values are inherent in theories of heredity that postulate a relatively stable genetic material and that negative values lurk in the supposedly humane Lamarckian view of a malleable genetic base. Noam Chomsky has recently taken the view that man's relative immalleability is a protection for him against potential tyrants:

> One can easily see why reformers and revolutionaries should become radical environmentalists, and there is no doubt that concepts of immutable human nature can be and have been employed to erect barriers against social change and to defend established privilege.
>
> But a deeper look will show that the concept of the "empty organism," plastic and unstructured, apart from being false, also serves naturally as the support for the most reactionary social doctrines. If people are, in fact, malleable and plastic beings with no essential psychological nature, then why should they not be controlled and coerced by those who claim authority, special knowledge, and a unique insight into what is best for those less enlightened?[59]

[59] Chomsky, *Reflections on Language* (New York, 1975), 132.

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This statement contains not only an observation on aspiring genetic manipulators of man, but also the concern of a more recent generation about the control of human behavior. From Chomsky's standpoint, both the eugenic selectionists of Germany and the Russian "makers of a new Soviet man" represented threats to human freedom of a fundamental sort. Yet Chomsky's observation, valuable though it is, presents only one more insight and does not attempt to give an ultimate analysis of whether putative theories about the mutability of man, genetic or behavioral, contain inherently positive or negative value. He admits that the theories can work both ways, with hypothetical immutability "employed to erect barriers against social change and to defend established privilege" and hypothetical mutability used "as the support for the most reactionary social doctrines." *Which* way the theories would work in a given historical situation depends on the values of the political and scientific authorities who would employ the theories and the associated technology.

In my opinion the present state of our knowledge of human heredity does not allow an abstract answer—that is, an answer apart from reference to available technologies and existing sets of social forces—to the question of whether available theories of the nature of man have in themselves positive or negative value content. But in given historical situations, such as those in Weimar Germany and Soviet Russia in the 1920s, rival scientific theories have invariably arisen within the context of given sets of social and political circumstances, of competing political ideologies and economic motives, and of existing systems of technological capabilities. Within those frameworks of external factors, rival scientific theories *do* have differential value implications; but they derive their value meaning much more from their relationships to these external considerations than from anything inherent in the science. In terms of impact, then, we are not asking the right question when we ask, "Do scientific theories contain in themselves social or political values?" Instead, we should pay more attention to what can be termed the "second-order" links between science and values—those which are contingent on existing political and social situations, current technological capabilities, and the persuasiveness of current ideologies, however flawed in an intellectual sense they may be.

If we look back at Germany and Russia in the 1920s with these overall extrascientific considerations in mind, it is clearly far from accidental that the societies eventually ended up on the opposite sides of the nature-nurture dispute with which we now associate them. Arguments for radical egalitarianism *could* be made more easily and more persuasively in accordance with Lamarckian environmentalism, and arguments for hierarchies of social and racial values *could* be placed more comfortably within the Mendelian system. The more obvious points of debate leaned in these directions, while the more subtle ones were not heard at moments of social and political trial. In this global, approximate, and intellectually flawed sense, then, these rival scientific theories did contribute heavily to arguments about values. These value implications derived from the relationships of the theories to their social and economic milieux, but that is normally the situation with science.

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Since I have maintained that the theories of human heredity current in Soviet Russia and Weimar Germany in the 1920s contained in themselves much less inherent value component than is often thought to be the case, my viewpoint might easily be considered as a buttress for the argument that science is really value-free. My interpretation might even be used to attempt to free scientists from a sense of responsibility to society. My reply to this misunderstanding of my analysis would be to point out that the value-free interpretation of these theories of human heredity is persuasive only if the links of science and society are severed and if science is viewed in abstract isolation from its setting. In fact, every scientific theory and every technological innovation *always* exists in a social and political setting, and the value impacts of these combinations can be massive.

Nuclear science is, seen in isolation, value-free. If, however, Nazi Germany had developed and employed atomic weapons in 1942, the configuration of our world might be very different today and the effects on our values would probably be incalculable. Scientific descriptions of our universe—for example, the Copernican or Ptolemaic alternatives—are, in the abstract, value-free, but the new and successful Copernican variant had a very large value impact when absorbed by European civilization at a time when the older variant was firmly interwoven with religion and culture. Societies have thrust upon them scientific theories and technological innovations which may either buttress or counter existing value preferences. The powerful groups in society are usually more successful than the weak ones in turning science and technology to their advantage, so there is a natural tendency for science and technology to buttress the values of ruling classes and political groups. In both Germany and Russia by the 1930s, these links between ruling value systems and contrasting theories of heredity had been successfully forged, after some initial uncertainties, and the links then became powerful weapons of propaganda.

The responsibility of scientists for their creations is not less because the value impacts of their work usually derive from a changing relationship between science and society, it is greater. They must not only try to judge what *can* be done on the basis of their work but what, in all probability, *will* be done in view of the existing social forces. Suppose, for example, that a scientist in Nazi Germany had discovered Tay-Sachs disease, a disorder with genetic causes that is more common among Jews than other groups of the population.[60] In this hypothetical situation, the decision whether to publish and to whom to send reprints (Hitler?) should become a moral act, not because of values inherent in the scientist's scholarly work, but because of the possible impact in that particular political setting of this purely scientific (not technological) finding.

ONE LAST WORD ON THE RELEVANCE OF THESE EPISODES to the present day. . . . I have emphasized that the value-contributions of rival scientific theories

[60] I am grateful to Noretta Koertge of Indiana University for suggesting this example.

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depend much more on external social forces and available technologies than they do on anything inherent in the theories. We now live in a time when the social circumstances and the available technology connected with the science of human genetics are very different from what they were in the twenties. We should not, therefore, assume that the value links of our time on the nature-nurture issue are identical to those of the 1920s. To ignore the genetic basis of human beings at the present time, when we understand that basis much better and are in some instances able to affect it in reliable ways or alleviate great suffering, would be quite inhumane. Eugenics is now a word in disrepute, but the use of genetic knowledge to benefit humankind is a far more viable possibility now than ever before. On the other hand, some of the dangers of the 1920s are still with us. We need to examine each issue relating to human genetics separately, considering the whole context of second-order value links, and decide the issues without *a priori* ideological commitments.

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On Lamennais, Chaadaev, and the Romantic Revolt in France and Russia

NICHOLAS V. RIASANOVSKY

In the name of the Father and of the Son and of the Holy Spirit. Amen.

Thy kingdom come.[1]

A SERIES OF CULTURAL SHOCKS marked the emergence of the romantic era in France, Russia, and other countries. Much has been written concerning the explosive impact of such works as Victor Hugo's *Hernani* or Michael Lermontov's *A Hero of Our Time*. Hugues-Felicité de Lamennais probably provided the greatest shock of all in the new romantic age in France with his *Les paroles d'un croyant* and Peter Chaadaev's first "Philosophical Letter" proved equally shocking in Russia. Lamennais wrote his brief, yet epochal, work in 1833 and published it in 1834. Chaadaev composed the first "Philosophical Letter" in 1829, but it did not appear until 1836 (and then through a censor's oversight) in Nicholas Nadezhdin's periodical, *The Telescope*. Lamennais had already acquired a great reputation as a religious writer and ideologist, in particular for his *Essai sur l'indifférence en matière de religion*, published in four volumes from 1817 to 1823; and he was in the process of a complicated evolution of thought and belief which had already attracted general attention. Moreover, his sympathizers, associates, and sometime disciples included not only such promising religious minds as Abbé Philippe Gerbet, Jean Baptiste Lacordaire, and the Comte de Montalembert, but the very cream of French romantic literary figures—Alphonse de Lamartine, Victor Hugo, Alfred de Vigny, Charles Sainte-Beuve, and others.[2] By contrast, Chaadaev did not have an

I want to thank the National Endowment for the Humanities for supporting my work for this study and my friends and colleagues for reading and improving the manuscript.

[1] These two statements—"Au nom du Père, et du Fils et du Saint-Esprit. Amen." and "Adveniat regnum tuum."—form, respectively, the first sentence of the text of Lamennais's *Les paroles d'un croyant* and the epigraph to Chaadaev's first "Philosophical Letter."

[2] Most major studies of Lamennais provide at least useful summaries of his central role in French religious, intellectual, and cultural life in the nineteenth century. See, for example, Jean-René Derré, *Lamennais, ses amis, et le mouvement des idées à l'époque romantique, 1824–1834* (Paris, 1962), esp. chap. 10: "Le mennaisisme et les grands romantiques," 529–614 (the identical book sometimes carries a different title: *Le renouvellement de la pensée religieuse en France de 1824 à 1834: Essai sur les origines et la signification du mennaisisme*, Thèse principale pour le Doctorat ès lettres présentée a la Faculté des Lettres et Sciences Humaines de Paris); F. Duine, *La Mennais: Sa vie, ses idées, ses ouvrages, d'après les sources imprimées et les documents inédits* (Paris, 1922); Alec R. Vidler, *Prophecy and Papacy: A Study of Lamennais, the Church, and the Revolution* (New York, 1954); and Louis Le Guillou, *L'évolution de la pensée religieuse de Felicité Lamennais* (Paris, 1966). See also

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established literary reputation in Russia, although he, too, belonged to his country's elite and numbered Alexander Pushkin among his friends. He therefore burst upon the Russian intellectual scene, almost appearing from nowhere (which is not meant to be a pun on his heading his letter from Necropolis). Chaadaev had read Lamennais and indeed seems to have been significantly influenced by him (although by the Lamennais of the early ultramontane and generally unbendingly Catholic period, not by that of *Les paroles d'un croyant*). Thus Chaadaev turned to the thought of restoration and reaction just when Lamennais, a leading exponent of that thought, was himself moving toward radicalism—an indication of a certain lag in Russian intellectual development. Lamennais, however, probably never heard of Chaadaev, who, like Lamennais, originally wrote his famous piece in French.

Les paroles d'un croyant created a sensation. According to Yves Le Hir, one of the closest students of Lamennais, it "was the greatest success of the age on the book market. Hundreds of thousands of copies spread throughout the world." Translations into many languages followed.[3] Pirated editions appeared almost immediately. "People waited in queues at reading rooms and paid so much an hour to read it; a group of students in the Jardin de Luxembourg was seen listening with enthusiasm" as it was read aloud. And several weeks after the book appeared, Montalembert wrote to Lamennais from Austria, "I never open a newspaper without seeing your name on the front page."[4] This extremist tract, a "gospel of insurrection" according to Lamartine, shook its readers, including most of Lamennais's friends; and it marked the decisive, final break between its author and Rome, although Lamennais still tried for a time to avoid that inevitable conclusion.[5] It destroyed a whole series of efforts at compromise between new intellectual orientations and the Catholic faith and significantly helped to create an abyss between a new generation of French romanticists and the pope. Beyond the shock of the moment, it pointed to Catholic liberalism, modernism, and Christian Socialism, to the twentieth—rather than to the nineteenth—century.[6]

such specialized studies as Christian Maréchal, *Lamennais et Lamartine* (Paris, 1907), *Lamennais et Victor Hugo* (Paris, 1906), *La Clef de "volupté"* (Paris, 1905) (on Lamennais and Sainte-Beuve); and Marc Citoleux, "Alfred de Vigny et La Mennais," *Annales de Bretagne,* 31 (July 1916): 301–22.

[3] Introduction to Hugues-Felicité Robert de Lamennais, *Les paroles d'un croyant* (Paris, 1949), v. This edition of the classic is the most scholarly and generally the best. For the immediate reaction to the work, see esp. Paul Vulliaud, *Les "Paroles d'un croyant" de Lamennais* (Amiens, 1928). Vulliaud states that the success of Lamennais's tract was beyond comparison to that of any other book (page 23), and he later quotes A. Duquesnel, "Never was so much commotion produced by a few pages reflecting the mind of a man" (page 99).

[4] Vidler, *Prophecy and Papacy,* 244.

[5] Le Hir quotes from Lamartine's letter on "the gospel of insurrection," disparaging—even despairing over—Lamennais's extremism, in the Introduction to *Les paroles d'un croyant,* 6. On Lamennais and Rome, see in particular his *Affaires de Rome* in F. de la Mennais, *Oeuvres complètes,* 12 vols. (Paris, 1836–37), vol. 12.

[6] See Joseph Ernest Renan's study of Lamennais, which served as the introduction to a new edition of two of Lamennais's works, *Le livre du peuple. Du passé et de l'avenir du peuple* (Paris, 1866), 1–74. In Renan's exaggerated estimate (itself already badly dated), "What Socrates was to philosophy in ancient Greece, Lamennais is to contemporary Catholicism: everything flows from him" (page 19). Or, as Vidler commented, Lamennais "was the founder, if anyone was, of modern ultramontanism, and of liberal Catholi-

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Chaadaev also earned sensation and scandal. In reducing Russia's historical role to zero, Chaadaev's first "Philosophical Letter" posed a direct, extreme challenge to the dominant official doctrine of crude and unmeasured glorification of the Russian way. And in a nihilistic *tour de force* the "Letter" aimed to shatter virtually all other convictions held by the Russian educated public, whether based on the optimism of the Age of Reason or on a belief in the sublime superiority of Russian Orthodoxy. Little wonder that the publication proved explosive and the outcry very loud.[7] While Lamennais was condemned by the Church, Chaadaev was officially declared insane by the state. In the judgment of Alexander Herzen, one of the very few sympathetic observers, "that letter was in a sense the last word, the dividing point. It was a shot that rang out in the dark night; whether it was something perishing that proclaimed its end, whether it was a signal or a cry for help, whether it heralded the dawn or foretold that it would never be—anyway, it forced all to awaken."[8]

As Herzen's evalulation suggests, Chaadaev's message became a point of departure for the great nineteenth-century historiosophic debate concerning the nature and destiny of Russia—a debate in which the Slavophiles and the Westernizers, and indeed Herzen himself, were to play leading roles. The message, moreover, even outlasted that debate and has remained one extreme perspective on Russia.[9] And, if criticism is the hallmark of the Russian intelligentsia, Chaadaev—this unusual "officer of the Hussars," to quote from Pushkin's well-known poem ("in Rome he would have been Brutus, in Athens Pericles, and here he is an officer of the Hussars"[10])—remains one of its most distinguished members (or forefathers if a later dating of the intelligentsia is preferred).

A COMPREHENSIVE SUMMARY OF THE CONTENT of *Les paroles d'un croyant,* one of the best known documents of modern European history, would serve little purpose. Nor would it be easy. The work, essentially a brief polemical tract, written at fever pitch in stunning Biblical French, overwhelmed its readers.

cism, and perhaps of Christian socialism, while he was pronounced to be an heresiarch and apostate by a church that has since appeared to adopt many of his ideas"; *Prophecy and Papacy,* 13. Nor has Lamennais's influence been seen only in terms of religious modernism or of radicalism. See, for example, Robert A. Nisbet's emphasis on Lamennais's importance as an early champion of pluralism in "The Politics of Pluralism: Lamennais," *Historical and Sociological Essays* (New York, 1970), 31–50 (originally published as "The Politics of Social Pluralism: Some Reflections on Lamennais," in the *Journal of Politics,* 10 [1948]: 764–86).

[7] I discussed Chaadaev's thought in its Russian context in *A Parting of Ways: Government and the Educated Public in Russia, 1801–1855* (Oxford, 1976), 167–76.

[8] As quoted in Raymond McNally, ed. and trans., *The Major Works of Peter Chaadaev: A Translation and Commentary* (London, 1969), 226.

[9] The view persists in the twentieth century, for example, explicitly in Alexander von Schelting, *Russland und Europa im Russischen Geschichtsdenken* (Berne, 1948) and at least implicitly in Richard Pipes, *Russia under the Old Regime* (New York, 1975). One poet has written, "the imprint Chaadaev left in the consciousness of Russian society is so deep and indelible that one cannot help but wonder, was it a diamond cutting glass?" Osip Mandelstam, *Sobranie sochinenii v dvukh tomakh,* 2 (New York, 1966): 326.

[10] "To P. Ia. Chaadaev's Portrait" in, for example, A. S. Pushkin, *Sobranie sochinenii,* 1 (Berlin, 1921): 196.

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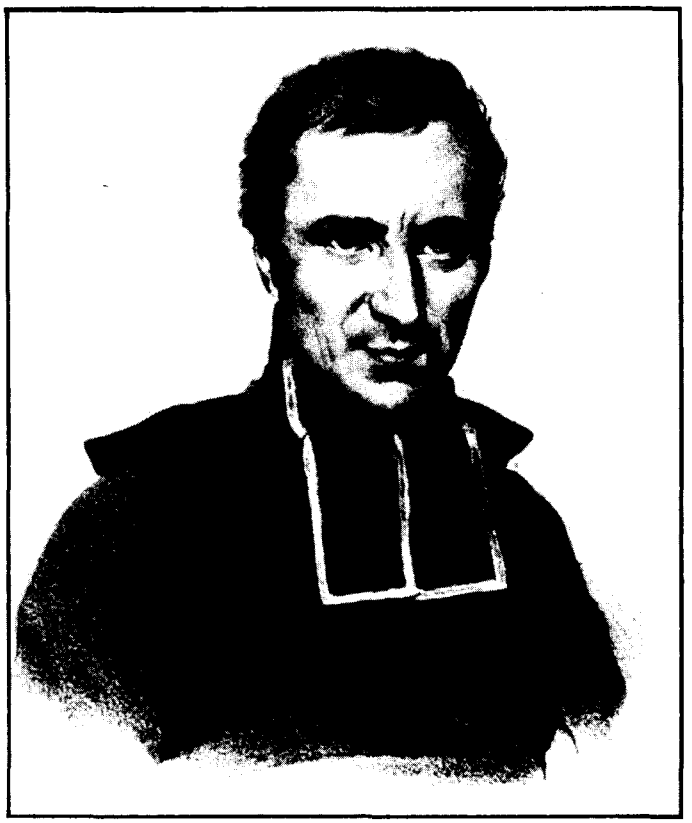
Peter Chaadaev. Lithograph, 1840s. Reproduced courtesy of Notre Dame University Press.

While few are in a position to judge Le Hir's assertion that its author was one of the nineteenth-century writers who understood best the genius of the Hebrew language, Lamennais certainly knew how to express himself in his own tongue and for his own age.[11] Arguing by image and association more than by logic and always addressing the people, Lamennais preached his radical doctrine: "Liberty is the bread which the peoples must win by the sweat of their brow." How can a man consider himself free when he has no plot of land to call his own, when his possessions, skills, and work can be appropriated by others, when he cannot worship and educate his children as he pleases, when he may not freely assemble to defend his rights and interests, when he cannot travel freely from place to place, when he can still be snatched from the bosom of his family in the night and thrown into a dungeon?[12] The rebellious priest insisted:

You have but one Father, who is God, and but one master, who is Christ. Therefore, when you are told of those who possess great power on earth, "there are your masters," believe it not. If they are just, they are your servants; if they are not, they

[11] Yves Le Hir, *Lamennais écrivain* (Paris, 1948), 225. And Renan referred to "fifty most beautiful pages in the grand style of our century"; Introduction to Lamennais, *Le livre du peuple. Du passé et de l'avenir du peuple,* 7.
[12] *Les paroles d'un croyant,* 260, 171–72.

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Felicité Lamennais. Nineteenth-century lithograph by Delpech.

are your tyrants. All are born equal: no one, coming into the world, brings with him the right to command. . . . On the balance of eternal law, your will weighs more than the will of kings: for it is peoples who make kings and kings are made for peoples, and peoples are not made for kings.[13]

Thus he tied his radical creed to man's position under God: "Our Heavenly Father did not shape the limbs of His children so they would be broken by chains, nor their souls so they would be murdered by servitude."[14] Lamennais wrote like a prophet seeing a vision:

These peoples and these nations woke upon their bed of agony and began to say to themselves: Wherefrom comes our suffering and out languidness, and the hunger and the thirst that torment us, and the chains that bend us to the ground and cut into our flesh? And their minds opened, and they understood that sons of God, brothers of Christ were not condemned to slavery by their Father, and that it was slavery that was at the source of all their ills. Each one then tried to break his chains, but none succeeded. And they looked at one another with great pity, and as love was acting in them they said: "We all have the same thought, why is it that we do not have a single heart? Are we not all sons of the same God and brothers of the same Christ? Let us save ourselves, or die together." And, having said that, they felt in themselves a divine

[13] *Ibid.*, 167, 168.

[14] *Ibid.*, 168.

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strength, and I heard their chains snap, and they fought six days against those who had enchained them, and on the sixth day they were victorious, and the seventh day was a day of rest. . . . And there were neither rich nor poor, but all had in abundance everything necessary for their needs, for all loved one another and helped one another like brothers. And a voice, like the voice of an angel, sounded from the sky: Glory to God Who gave intelligence, love and strength to His children. Glory to Christ Who restored freedom to His brothers.[15]

Chaadaev's first "Philosophical Letter" had more in common with Lamennais's *Les paroles d'un croyant* than has been generally realized. The Russian also believed—on the basis of largely the same intellectual background—in Christianity as an active, decisive, social force working its way in history. He wrote that in Christian society "everything must necessarily lead towards the establishment of a perfect order on earth," for "otherwise our Lord's word would be denied by the facts." It was this active Christian, more specifically Roman Catholic, principle that constituted for Chaadaev the power and the meaning of the West: "Thus, despite all that is incomplete, vicious, evil, in European society as it stands today, yet it is nonetheless true that God's reign has been realized there in some way, because it contains the principle of indefinite progress and possesses germinally and elementarily all that is needed for God's reign to become established definitely upon earth one day."[16] Even the content of Chaadaev's social message might have been at times close to that of Lamennais; numerous scholars, particularly Soviet specialists, have persistently tried to present the strange Russian philosopher as a kind of revolutionary. They have been aided in their efforts by Chaadaev's many ambiguous, aphoristic, and oracular pronouncements and by a peculiar proclamation, not published until 1935, in which the author—evidently Chaadaev—tried in awkward, pseudopopular language to inform the Russian populace about the uprisings, presumably the revolutions of 1848, in the West. That brief document was almost certainly a secret literary exercise rather than a real attempt at agitation or propaganda. The peoples of Europe, it concluded, were saying, "We do not want our tsars, our sovereigns. . . . We do not want to obey them." The tsars "have oppressed us, enslaved us, often they made us drink the bitter chalice to the end. We want no other tsar, except the Heavenly Tsar."[17]

Whatever the social veiws of Chaadaev in different periods of his life, the first "Philosophical Letter" was not concerned primarily with social problems, but with a still larger, nationally oriented, cultural issue. Whereas Lamennais appealed to the peoples of Europe, heretofore excluded by oppressive rulers and the upper classes from the benefits of European progress, Chaadaev commented on what he considered to be the total exclusion of

[15] *Ibid.*, 129.

[16] McNally, *Major Works of Peter Chaadaev*, 41, 47.

[17] As quoted in D. Shakhovskoi, "Neizdannyi proekt proklamatsii P. Ia. Chaadaeva 1848 g.," *Literaturnoe Nasledstvo*, 22–24 (1935): 680. Shakhovskoi reminds the reader (page 681) that the last sentence of the proclamation exactly corresponds to statements in the so-called "Orthodox Catechism" written by Chaadaev's friend, the Decembrist Serge Muraviev-Apostol, as a plea for soldiers to rebel.

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Russia from that same progress. That comment assured him a permanent place in Russian intellectual history.

> One of the most deplorable things in our unique civilization is that we are still just beginning to discover truths which are trite elsewhere—even among people less advanced than we are in certain respects. We have never advanced along with other people; we are not related to any of the great human families; we belong neither to the West nor to the East, and we possess the traditions of neither. Placed, as it were, outside of the times, we have not been affected by the universal education of mankind. What has long since constituted the very basis of social life in other lands is still only theory and speculation for us. . . . Do we not all have one foot in the air? There is no definite sphere of existence for anyone. . . . In our houses we are like campers; in our families we are like strangers; in our cities we are like nomads, more nomadic than the herdsmen who let their animals graze on our steppes, for they are more bound to their deserts than we to our cities.[18]

The Russians had missed the stage of adolescence, which is crucial in the life of a people. During adolescence a people acquires "its most vivid recollections, its myths, its poetry, all its strongest and most fertile ideas." For Chaadaev, these were the "necessary bases of society."[19]

> The epoch of our social life which corresponds to this moment was filled by a dull and somber existence without vigor and without energy, in which the only thing that animated us was crime, the only thing that pacified us was slavery. No charming memories and no gracious images live in our memory, no forceful lessons in our national tradition. . . . We live only in the most narrow kind of present without a past and without a future in the midst of a shallow calm.[20]

Chaadaev reiterated the analogy between the life of a person and the life of a nation: just as it "takes years to educate" an individual, it "takes centuries to educate" a people. Russia, to him, did "not seem to form an integral part of humanity"; it did not develop; it existed "only to provide some great lesson for the world."[21] Thus,

> for us historical experience does not exist; ages and generations have flowed by fruitlessly for us. It would seem that in our case the general law of humanity has been revoked. Alone in the world, we have given nothing to the world, taken nothing from the world, bestowed not even a single idea upon the fund of human ideas, contributed nothing to the progress of the human spirit, and we have distorted all progressivity which has come to us. Nothing from the first moment of our social existence has emanated from us for man's common good; not one useful idea has germinated in the sterile soil of our fatherland; we have launched no great truth; we have never bothered to conjecture anything about ourselves, and we have only adopted deceiving appearances and useless luxury from all the things that others have thought out.[22]

One "great man" (Peter) had attempted—and failed—to civilize Russia. Later, "another great Prince" (Alexander I) marched his armies across

[18] McNally, *Major Works of Peter Chaadaev*, 27–28.
[19] *Ibid.*, 29.
[20] *Ibid.*, 30.
[21] *Ibid.*, 32.
[22] *Ibid.*, 37–39.

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Western Europe, but the Russians embraced only the "evil ideas and fatal errors" which led to the Decembrist rebellion, "an immense calamity which threw us back a half a century." Russia had a disease of the blood, of the spirit, which alienated "any real progress."[23] The secret of Russia's sterility lay in its schismatic separation from the universal Christian civilization of the West. Certainly Russians were Christians, but "the fruit of Christianity did not mature" for them. Russia remained outside the European community of nations. For over fifteen centuries Europeans had "only one single idiom with which to address God, only one single moral authority, only one single religious conviction. . . . it is Christianity which has produced everything over there."[24]

LAMENNAIS AND CHAADAEV CAN BE CONSIDERED outstanding representatives of the romantic rebellion in their respective countries. To be sure, as befits pioneers of a new world view, they were transitional figures. They were also striking and unusual individuals whose remarkable personalities help account for their contributions. But both men went beyond intellectual tradition and personal idiosyncrasy to formulate novel messages which found resonance among their contemporaries.

Born in 1782, Lamennais had distinguished himself as one of the founders of modern ultramontanism before he became the great religious rebel of the age. He had been a proponent and defender of one of the oldest and most important institutions in Western civilization, the Roman Catholic Church, and of one of the central assumptions of that Church—the special position and power of the pope. Yet Lamennais's argument, based on Vicomte Louis de Bonald's new conception of language and the philosophy of *sensus communis* (in contrast to the scholasticism and Cartesian rationalism then prevalent in theological schools) bothered Rome even before Lamennais's radicalism became an issue. In his ultramontane period Lamennais had already come to believe in the liberal doctrine of the separation of church and state, while his emphasis on *sensus communis* pointed to the later hypostasization of the people. So dramatic on the outside, Lamennais's evolution had a remarkable inner consistency, continuity, and logic. Indeed, it is impossible to tell when he became intellectually a romanticist, unless—as a number of scholars have asserted—the new theology of the Bourbon Restoration was already romanticism.

Before he staged his rebellion, Chaadaev's credentials had been much more modest than Lamennais's. He too, however, had undergone an important intellectual evolution. Recent research has established that Chaadaev, born around 1794, possessed two libraries. The first, assembled during the first two decades of the nineteenth century and sold in 1821, was typical for a post-Enlightenment Russian intellectual in its rationalist orientation, its "Vol-

[23] *Ibid.*, 39.

[24] *Ibid.*, 43–44.

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tairianism." But the second library, acquired in the 1820s during a stay of several years in Western Europe and in Russia, marked a new departure. Raymond J. NcNally has classified the authors of that library into seven categories: "1) French Romantic social thinkers and historians, 2) Idealist philosophers, 3) English and Scottish empiricist philosophers, religious writers, and literary writers, 4) German church historians, biblical scholars, and literary writers, 5) Christian mystics, 6) Rationalists, and 7) Classical authors."[25] Chaadaev learned the contents of his second library so well before he stated his own views in the late 1820s that the chronological divide between the two libraries constitutes part of the nebulous and uncertain border between Russian Enlightenment and Russian romanticism.

Chaadaev's first "Philosophical Letter" is a study in dissolution. The Russian past, the Russian present, and even physical reality itself dissolve into nothingness. "I cannot stop being dumbfounded by this void and this surprising solitude," he averred. "There is no definite sphere of existence for anyone, no good habits, no rule for anything at all; not even a home, nothing which attracts or awakens our endearment or affections, nothing lasting, nothing enduring; everything departs, everything flows away, leaving no traces either without or within ourselves."[26] The "Letter" is written from "Necropolis," and its crucial and most frequently repeated concept, its focal point, is "nothing." It would seem that Chaadaev was on the verge of suicide when he composed his first "Philosophical Letter."[27] By contrast, *Les paroles d'un croyant* is not the vision of a would-be suicide, nor does it possess the devastating psychological unity of the first "Philosophical Letter." Yet *Les paroles d'un croyant*, too, has an obsessed and strange quality which cannot be entirely separated from its author's innermost perception and struggle.

In the romantic era in particular, however, individual psychology merged with the spirit of the age. Romanticism has frequently been described as more an embodiment of feeling than of thought, more a sensibility than an ideology. Such common romantic characteristics as bombast, exaggeration, general lack of measure, and accentuated sense of the tragic and the desperate abound in both *Les paroles d'un croyant* and the first "Philosophical Letter." Were it not that Lamennais and Chaadaev write very well and mean what they say, both works might easily have been parodies of themselves. More importantly, both essays exhibit what has often been postulated as the central focus and paradox of romanticism: its abandonment of old moorings in a heroic search for values. That search, scholars have argued, was inevitably doomed to failure; for values once abandoned could not be "invented," and faith once broken could not be restored. As Emile Faguet has written of de Vigny, "no one knew so fully, so constantly as he the real torture of melancholy which is to worship

[25] Raymond J. McNally, *Chaadayev and His Friends: An Intellectual History of Peter Chaadayev and His Russian Contemporaries* (Tallahassee, 1971), 170. For comprehensive information on the two libraries, see *ibid.*, 164–98.

[26] McNally, *Major Works of Peter Chaadaev*, 39, 28.

[27] On Chaadaev's life, especially see Charles Quénet, *Tchaadaev et les lettres philosophiques: Contribution à l'étude du mouvement des idées en Russie* (Paris, 1931). In the opinion of some scholars, Chaadaev did take his own life.

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an ideal and not to believe it. For him it was equally impossible to believe in glory, love, happiness, and God and not to love glory, love, happiness, and religion."[28] Of these ideals, inaccessible yet necessary for the romanticists, the religious one was crucial. The romantic movement passed under the sign of "the quest for religion and the inability to embrace it."[29]

If romanticism is approached as a problem of belief, Chaadaev's first "Philosophical Letter" provides an excellent instance of the nihilism present in the new world view and, indeed, intrinsic to it.[30] Insofar as the nature and fate of Russia were concerned, Chaadaev had only one word to offer: "nothing."[31] Nor did it end there. As a number of scholars have noted, although the first "Philosophical Letter" postulated the primacy of the religious principle in human history, neither it nor its author seemed infused with much religious feeling. Paradoxically perhaps—but very much in line with the romantic paradox—the Christian character of Chaadaev's outlook came into question.[32] Lamennais's Christian integrity has also been challenged, and everything from his hesitation to become a priest to his eventual break with the Roman Catholic Church have been cited to condemn him. To some he represents the very essence of romantic pseudoreligion—"the promise and the unfulfillment of the Romantic revival of Christianity."[33] But even given a recognition of the conviction and power of *Les paroles d'un croyant* and, furthermore, a sympathy for Lamennais's entire religious evolution, that development belonged to the new romantic age. Beginning as the great champion of the ultramontane Right, Lamennais first moved to the liberal and, in effect, modernist Catholic Left and finally outside the Roman Catholic Church and,

[28] Faguet, *Etudes littéraires sur le dix-neuvième siècle*, 5th ed. (Paris, 1889), 128.

[29] H. Schenk, *The Mind of the European Romantics: An Essay in Cultural History* (London, 1966), 76. Schenk's study is an excellent example of recent religious (in effect Roman Catholic) critiques of romanticism. For an earlier, much cruder, but also expert example, see A. Viatte, *Le Catholicisme chez les Romantiques* (Paris, 1922).

[30] On nihilism, suicide, and death in the romantic movement, see, for example, Schenk, *Mind of the European Romantics,* esp. 49–65.

[31] In a subsequent essay, "The Apology of a Madman," Chaadaev exchanged this psychologically unbearable position for a positive assertion of the decisive, beneficial role of Peter the Great.

[32] For an extreme but very authoritative judgment, see Georgii Florovskii, *Puti russkogo bogosloviia* (Paris, 1937), 247.

[33] Schenk, *Mind of the European Romantics*, 115. According to Schenk, "nothing could characterize the dual nature, and indeed the inner contradictions of Romanticism better than the fact that the foremost Romantic herald of the Christian revival, the Abbé de Lamennais, was himself violently torn between faith and unbelief. Nor was this true only of certain periods in his life, as had been the case with some of the greatest religious figures of the past. In marked contrast to St. Augustine or Pascal, Lamennais it seems never experienced the blissful certitude of unshakeable faith." "More than any of his contemporaries," Schenk remarked later on, Lamennais "embodied Romanticism, that latest European-wide movement of ideas and sentiments. The brilliant and sometimes ecstatic style; the inspired and almost prophetic vision; the yearning for a faith and the religious frustration; the optimism regarding the fate of mankind paradoxically combined with the sombre mood of *Weltschmerz*; the propensity for meditation marred by an incurable restlessness of mind; the stirring promise and the disheartening unfulfillment—the whole gamut of Romantic *leit-motivs* are present in Lamennais, many of them carried to their extreme and revealing the forcefulness and depth and, at the same time, the inner disharmony of a movement that was so fervently longing for harmony without ever achieving it." See *ibid.*, 81, 121. For Viatte, "if ever the romantic hero was incarnated in a being of flesh and bones, it was in Lamennais," for he symbolized "better than anyone else the fate of romanticism"; *Le Catholicisme chez les Romantiques,* 133, 141. But Viatte considered Lamennais perfectly sincere, a romantic enthusiast who ardently and rapidly changed his views.

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perhaps, outside Christianity.[34] The emphasis on "the people," so characteristic of the later Lamennais, formed part of the new romantic creed spreading all over Europe and ranging in expression from works of German idealistic philosophy to the novels of Victor Hugo and George Sand and the historical narratives of Jules Michelet. In Russia, too, the new dogma of the people was to find manifold expression—not in the thought of Chaadaev, who remained unreconstructed in that respect—from Vissarion Belinskii's seminal literary criticism and Herzen's brilliant political and social polemics to the teachings of the Slavophiles. Indeed, the Slavophiles gave the romantic concept of the people its most powerful intellectual affirmation in Russia.[35]

As he considered Lamennais's remarkable evolution, Sainte-Beuve had good reason to note that the priest's development was, more or less, "the story of each one of us" in the France of the July Monarchy. Lamennais's tribulations—"his astonishing vicissitudes"—are "typical of what any number of us have been fated to experience."[36] Still Sainte-Beuve was not entirely accurate; differences remained. Lamennais was, after all, a priest and theologian, a leading defender of the Roman Catholic Church; his literary friends and acquaintances like Sainte-Beuve had no comparable background or commitment. Their religious search, therefore, revealed all the more strikingly the winds of the season, the romantic moods of the moment. (Often personally tragic, their quest also presented on occasion certain aspects of caricature.) Not only Lamartine and Hugo (whose confessor Lamennais became) but also, to a lesser extent, Sainte-Beuve and even the bitterly sceptical de Vigny, as well as others, linked their faith or their search for faith to the golden

[34] On the religious evolution of Lamennais after *Les paroles d'un croyant,* see Le Guillou, *La pensée religieuse de Felicité Lamennais.*

[35] On the Slavophiles, see—in addition to my *A Parting of Ways: Government and the Educated Public in Russia*—my *Russia and the West in the Teaching of the Slavophiles: A Study of Romantic Ideology* (Cambridge, Mass., 1952); Andrzej Walicki, *The Slavophile Controversy* (Oxford, 1975); and such studies of individual Slavophiles as A. Gratieux, *A. Khomiakov et le mouvement slavophile,* vol. 1: *Les hommes,* vol. 2: *Les doctrines* (Paris, 1939); Peter K. Christoff, *An Introduction to Nineteenth-Century Russian Slavophilism: A Study in Ideas,* vol. 1: *A. S. Komjakov,* vol. 2: *I. V. Kireevskij* (The Hague, 1961–72); Eberhard Müller, *Russischer Intellekt in europäischer Krise: Ivan V. Kireevskij (1806–1856)* (Cologne-Graz, 1966); and A. Gleason, *European and Muscovite: Ivan Kireevsky and the Origins of Slavophilism* (Cambridge, Mass., 1972). In a sense, Alexis Khomiakov, the Slavophile theologian, was, like Lamennais, a forerunner of religious modernism. His brilliant religious thought, more appreciated and influential in the twentieth than in the nineteenth century, always stayed within the doctrinal limits of Orthodoxy to which Khomiakov, himself a layman, remained unreservedly devoted. But, as perceptive observers noted from the start, that thought, though theologically unexceptionable, was highly selective. Based on love in Christ rather than authority, it stressed the role of the community of believers and not of the ecclesiastical hierarchy, and it was more immanent than transcendental: "The Church is not authority, just as God is not authority, just as Christ is not authority; because authority is something external to us. Not authority, I say, but truth, and at the same time the life of a Christian, his inner life; for God, Christ, the Church live in him a life which is more real than the heart which beats in his breast, or the blood which flows in his veins; but they live in him only inasmuch as he himself lives an ecumenical life of love and unity, that is, the life of the Church." Khomiakov, *Polnoe sobranie sochinenii,* 8 vols. (Moscow, 1900–14), 2:53 (the second volume assembles Khomiakov's religious writings). Khomiakov's crucial theological concept of *sobornost*—an association of believers in truth, love, and freedom—certainly seems to be related to romantic organicism and adoration of the people; see my "Khomiakov on *Sobornost*" in *Continuity and Change in Russian and Soviet Thought,* ed. Ernest J. Simmons (Cambridge, Mass., 1955), 183–96. In theology not only Khomiakov entirely but Lamennais very largely were autodidacts.

[36] As quoted in Vidler, *Prophecy and Papacy,* 12.

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eloquence of the Breton preacher: for some of them—for example, Lamartine if not de Vigny—Lamennais's "justice," "humanity," and "the people" seemed to give a new meaning to religion, where traditional Catholicism no longer carried conviction. Thus they were affected by his evolution and his ultimate fate. The crash was shattering. Papal condemnation, first of the doctrines of *L'Avenir*—Lamennais's journal around which liberal French intellectuals had rallied under the slogan of "God and Freedom"—and finally of the master's *Les paroles d'un croyant,* made religious rapprochement and compromise impossible. Although it is dangerous to overestimate Lamennais's influence on so many literary figures (one suspects that their lives would have been essentially the same without Lamennais), their bid for a new religious outlook was tied to that of the remarkable priest; and subsequent years brought for most of them unrequited longing, pessimism, and despair.[37]

As elsewhere, the same religious search and longing also occurred in Russia. The celebrated conversion of Ivan Kireevskii, the philosopher of Slavophilism, provides a notable example; as he looked at a miracle-working icon, he suddenly came to realize that it must have been magnetized by the faith and the prayers of the common people. Although Chaadaev's own religious beliefs remain an enigma, the pattern of the great French romanticists was largely repeated, with a slight delay in time, among the Russian Westernizers for whom the issue of the existence of God did much to split the radicals from the moderates and to separate such friends as Alexander Herzen and Timothy Granovskii.

FOR ALL OF THE SIMILARITIES SCHOLARS CAN DRAW between Russian and French romanticism and for the number of parallels apparent in the styles of Lamennais and Chaadaev, the contents of the first "Philosophical Letter" and *Les paroles d'un croyant* were quite different. Chaadaev devoted his essay to a single theme: the historical nullity of Russia, especially in contrast to the West. On the one hand, the "Letter" reflected the perennial sensitivity of educated Russians—relative newcomers to modern Western civilization—to their reception by that culture and their position in that society. And, on the other, it reacted to changing historical reality, to new intellectual tastes and preferences. The Voltairian view of Russia as a brilliantly successful disciple

[37] Although there were many nuances to the relationship between Lamennais and other French intellectuals and although some of these relationships—for example, Lamennais's connections with Chateaubriand—were extremely complex, scholars still find it possible to generalize along the following lines: "Lamennais summoned Gregory XVI to put himself at the head of liberated peoples. All anxiously waited the answer, and many, personifying in Lamennais the living force of the Church and joining him as the only man able to modernize Orthodoxy, dreaded the decision of the pope as a writ of life or death. . . . And when the condemnation which proscribed liberal ideas came, how many souls, hesitant until then, abandoned belief as no longer compatible with contemporary reality." Viatte, *Le Catholicisme chez les Romantiques*, 187. The literature on the subject is enormous, for it includes not only the works of some of the best known French writers but also the vast literature on those figures. My favorite study, for its perceptive and sensitive treatment of religion and irreligion, is the Marquis de Luppé's *Les travaux et les jours d'Alphonse de Lamartine* (Paris, 1942). Also see Georges Bonnefoy's enormously meticulous—and alas posthumous—investigation of de Vigny: *La pensée religieuse et morale d'Alfred de Vigny* (Paris, 1946).

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of the Enlightenment or the near-universal admiration and hope aroused by Alexander I at the time of the "liberation of Europe" and the Congress of Vienna[38] no longer dominated the intellectual scene. In the increasingly romantic climate of organic nationalism, a new accounting had to be given of the meaning of Russia and of its relationship to the West. Chaadeav's answer was singularly his own. But it was relevant enough to the concerns of Russian intellectuals of the time to produce shock and scandal and, more importantly, to serve as a starting point for the great historiosophical debate between the Slavophiles and the Westernizers.

Slavophilism, the most comprehensive and coherent romantic teaching to appear in Russia, can in a sense be considered a sweeping rejection of the first "Philosophical Letter" and of the subsequent "Apology of a Madman." In direct opposition to Chaadaev's historical nihilism, the Slavophiles postulated a glorious and meaningful Russian past. And, in contrast to Chaadaev's later eulogizing of Peter the Great as the man who brought Russia into being, the Slavophiles regarded the reforming emperor as an evil genius who turned his native land in the false Western direction. They countered Chaadaev's praise of the Catholic Church with an elaborate affirmation—even a creative theological affirmation—of Orthodoxy and with a concomitant attack on Rome. But they were similar to, as well as the opposite of, Chaadaev. Above all, both Chaadaev and the Slavophiles theorized within essentially the same framework of romantic historiosophy. Metaphysical principles determined historical development. For better or for worse, Peter the Great changed the destiny of Russia. Chaadaev on the one hand and Alexis Khomiakov and his associates on the other extolled religion and its decisive significance in the life and history of man. The Westernizers, to some extent the heirs apparent to Chaadaev's thought, directly drew upon Chaadaev's later championing of Peter the Great and all for which he stood—the Westernization of Russia. But the Westernizers abandoned that emphasis on religion and religious principles so fundamental to Chaadaev's historical reasoning.

That Lamennais's complaint in his great work was not at all the same as Chaadaev's lament was much more than a matter of chance. To be sure, the French theologian might have been able to accept the Moscow philosopher's view of Russia, which Lamennais detested as a brutal oppressor of the Catholic Poles. But he could not have made a similar analysis of his own country. For in the general view of that age Russia could be dismissed as an enigma, put at the bottom of civilization or outside it, while France belonged at the top. In France that classification acquired the power of a fundamental conviction, held usually in a remarkably matter-of-fact way; *la grande nation* had no equals and certainly no superiors. Of course, the French also had their problems, although they were very different from those Chaadaev saw for Russia. The superior French position in the world demanded maintenance

[38] Sadly enough, Rouget de Lisle, the creator of "La Marseillaise," was among those who eulogized the Russian emperor, imploring him to "restore to the Bourbons their throne, to the lilies their splendor"; Charles Corbet, *L'Opinion française face à l'inconnue Russe (1799–1894)* (Paris, 1967), 95.

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and emulation. The restored Bourbons and particularly the bourgeois king, Louis-Philippe, could never match the glory of Napoleon. The victorious nation in arms of the French Revolution could not be recreated in 1870. In general, the boredom and disaffection, apparently so widespread during the July Monarchy, especially in the 1840s, were linked directly to France's heroic past and its heroic expectations. If Russians were worried, even at times obsessed, by feelings of stagnation, the corresponding French fears were of losing control and falling from the summit.

Unconcerned with the historical and cultural standing of his country, which was Chaadaev's main preoccupation, Lamennais had other anxieties as he was writing *Les paroles d'un croyant*. His relation to Rome, his very ability to remain within the Catholic Church continued to be a pressing problem, although Lamennais ostensibly ignored that entire subject in his new work. He believed—or at least desperately wanted to believe—that, as long as he dealt with society and politics rather than theology, he could avoid new conflicts with Rome. *Les paroles d'un croyant*, however, did represent a major challenge to Church authorities—the episcopate and curia—and was so interpreted by virtually everyone but Lamennais himself. His essay belied his claims to the contrary; it bespoke an irreparable break with the Church.

Why no such challenge or break occurred in Russia raises a useful question for historians—although it may be quixotic to seek explanations for that which did not happen in history. Yet an assumption of this entire study has been that educated Russians and educated Frenchmen of the first half of the nineteenth century lived in basically the same intellectual world. More specifically, Khomiakov's theology as well as Lamennais's could lead to modernism. Institutionally and culturally the Orthodox Church in Russia and the Roman Catholic Church in France had much in common. In France as in Russia specialists claim that the Church was depressed, stagnant, traditionally scholastic, and even nonintellectual, except, perhaps, during a revival at the end of the July Monarchy. And they stress that laymen—Chateaubriand, de Maistre, de Bonald, or Montalembert—provided whatever creative vitality the French Church possessed.[39] The differences, however, were perhaps decisive. The relative freedom of expression in France on religious as well as on political and social issues did not exist in Russia. Had *Les paroles d'un croyant* been written in the empire of the tsars, it could not have been published there. Nor was submission to Rome relevant.[40] More broadly, the weak Orthodox Church in Russia, in contrast to the strong churches in the West, particularly the Roman Catholic Church, had little effect on Russian intellectuals and therefore rarely became the target of their revolt. Also, the Orthodox and Roman Churches exhibited different degrees of stagnation. Though archaic

[39] On all of these points, see, in particular, Charles Pouthas, *L'Eglise et les questions religieuses sous la monarchie constitutionnelle (1814–1848)* (Paris, 1961).

[40] It was certainly relevant in Russian Poland, but that is another story. Incidentally, some of the sharpest differences between the radical Lamennais and the conservative Pope Gregory XVI were over papal policy regarding Poland.

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and backward, the Church in France, some would insist, was much more advanced than the Church in Russia, which could produce neither a Lamennais nor his revolutionary message. In any case, whether because of the greater backwardness of the country, because of the fewer real restrictions the Church imposed on Russian intellectuals, or for other reasons, Chaadaev's rebellion, in contrast to that of Lamennais, took a generally cultural, not a religious, form.

Still, in spite of the papal reaction to *Les paroles d'un croyant,*[41] Lamennais was correct in declaring that his essay did not deal with Rome. In fact, he appealed directly to the people in the name of a social transformation, apparently by means of a mass rebellion.[42] As at the time of Creation, six days of activity (in this case of combat) were to precede the fulfillment and the seventh day of rest. Authorities, ranging from kings (seven of whom appeared in a dreadful vision on the pages of the book) to economic exploiters, "who have no name except in hell," were thus to be swept away. Nor did the angry priest waste words in disposing of most of the institutional pillars of the established order. "Bad laws form another cause of endless dissensions. And there are no laws in the world except bad ones. What other laws does he who has the law of Christ need?"[43] There was enough here to terrify even the bravest of hearts.

In contrast to Chaadaev's position in Russia, moreover, Lamennais never stood quite alone. Rather he, and specifically *Les paroles d'un croyant,* belonged to the well-established French radical tradition (or, more exactly, to a whole series of traditions). There was, of course, the great French Revolution itself—not to mention the July Revolution of the immediate past—to prove beyond doubt that government and society could be changed by violence. There was the more egalitarian branch of French radicalism associated with François Babeuf and "the conspiracy of equals," as well as with the Carbonari and Auguste Blanqui. There was the Utopian Socialist literature produced by such masters of the genre as Saint-Simon and Charles Fourier. There was the great interest in the masses exhibited in the writings of George Sand and others at the time Lamennais was composing *Les paroles d'un croyant.* There were strikes and conspiracies in Paris and actual fighting in the streets in

[41] The papal encyclical, *Singulari nos,* described *Les paroles d'un croyant* as "small in size but immense in its perversity" and emphasized that it "criminally urged the people, abusing impiously the Word of God, to break entirely the bonds of public order, to overturn all authority, to excite, feed, spread, and strengthen sedition, disturbances, and rebellions in the empires. . ."; la Mennais, *Oeuvres complètes,* 12: 389, 395. Volume 12 contains both the Latin text and a French translation; see pages 386–401.

[42] Lamennais's definition of "people" ("peuple") tended to equate "peuple" and "prolétaires." The proletariat possessed nothing and thus depended on its own labor for existence. See, for example, *De l'esclavage moderne,* first published in 1839. At times he described "the people" as "the dominated—as opposed to the dominating—class, the class politically enslaved as opposed to the class politically free"; *Le livre du peuple. Du passé et de l'avenir du peuple,* 134. On other occasions he broadened the definition to include all those productively employed, all who contributed something to society (i.e., all but the idle rich): "Subtract a small number of the privileged devoted exclusively to the pleasures of life, and the people is humanity"; *ibid.,* 95. Once Lamennais estimated—using a narrower definition of "people"—that "out of about a billion individuals who compose humankind, more than nine hundred million belong to the people"; *ibid.,* 196.

[43] *Les paroles d'un croyant,* 211.

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Lyons.[44] Thus Lamennais's explosive book came out of the French radical tradition; his work in turn also contributed greatly to it. More precisely, to the great French political debate Lamennais contributed the inspiration and the power with which he stated radical views (which affected particularly the young[45]) as well as a most striking and successful blend of the religious and the political message. Some Frenchmen in search of political and social change had already sought religious sanctions of one kind or another, but never was the appeal so forceful and so direct. It was this incendiary introduction of religion into politics that profoundly shook the pope and other readers of the irrepressible French priest. Lamartine wrote of "the gospel of insurrection" and of "Babeuf deified"; Bautain, of the "*Social Contract* presented in the form of an apocalypsis."[46] Like Lamartine, a few deplored the extremism of the work while retaining a high general regard for Lamennais and his ideas; more often, the wild enthusiasm of some matched the total condemnation of others.[47] Indeed, the lines on the issue of Christianity and revolution that were sharply drawn after the publication of *Les paroles d'un croyant* are very much with us today—from the Vatican to Africa and Latin America.

But some passages in *Les paroles d'un croyant* are decidedly moderate. To enhance this moderate tone, moreover, at the end of May 1834 Lamennais wrote a new chapter, which he inserted in the fourth and subsequent editions of the work. He announced that "to pillage, to steal means to attack the poor as well as the rich; it means to overturn the foundation of all social life among men. . . . Whoever possesses nothing can come to possess something only because others already possess, because they can give him something in exchange for his work." He claimed, "Order is the good, the interest of all." And he eschewed illegality, warning, "do not drink from the cup of crime; at

[44] *Les paroles d'un croyant* was also a very briefly sketched, oracular, and elliptical utopia. It lacked that huge, pseudoscientific utopian mechanism which so preoccupied Fourier and others all their lives; but its spirit was that of a new heaven and a new earth, of justice and blessedness for all. It is worth noting that Fourier, Lamennais, and many of their contemporaries began their reasoning with the assumptions that God was good, that He wanted man to be happy, and that, therefore, a just and happy social order could and should be achieved. For my views on Fourier, see *The Teaching of Charles Fourier* (Berkeley and Los Angeles, 1969). For an original recent contribution to the examination of the general spirit of rebellion in France in the early 1830s, see Anthony Esler, "Youth in Revolt: The French Generation of 1830," in Robert J. Bezucha, ed., *Modern European Social History* (Lexington, Mass., 1972), 301–34. Chaadaev, of course, lacked this radical and revolutionary background for his work, except for the abortive aristocratic (although ideologically liberal and in part even Jacobin) Decembrist rebellion. Behind France in its economic development, Russia had as yet no pressing "social problem" of an industrializing society. Serfdom and the poverty of the masses were apparently different matters; and for various reasons a radical critique of serfdom was slow to develop in Russian literature, except for the early instance of Alexander Radishchev's *Journey from Petersburg to Moscow* (1790). And the Russians never confused the two problems: the Slavophiles, for instance, were much concerned with serfdom in their native land but were happy at the same time that Russia was spared the "social problem" of the West.

[45] For the impact of *Les paroles d'un croyant* on the young, see, for example, Vulliaud, *Les "Paroles d'un croyant" de Lamennais*, 95–96.

[46] Le Hir quotes Lamartine in the Introduction to *Les paroles d'un croyant*, 6; and Vulliaud quotes Bautain in *Les "Paroles d'un croyant" de Lamennais*, 123.

[47] See, for example, Monseigneur Guillon's critique in Vulliaud, *Les "Paroles d'un croyant" de Lamennais*, 152; and generally see *ibid.*, *passim*.

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the bottom of it there is bitter distress, and agony, and death."[48] In other chapters, too, Lamennais issued injunctions against radical socialism: "Everyone has the right to keep what he has, without which no one would possess anything."[49] Even the celebrated call to rebellion against unjust authorities he qualified; he sanctioned rebellion only "if there is no reason to fear that more misfortune will result from it."[50] So put, François Guizot or even Louis-Philippe could well approve certain rebellions, although the Russian government was bound to disagree.[51]

More significant than the extent of agreement or disagreement between Lamennais and other educated Frenchmen on various points is the fact that the main argument of *Les paroles d'un croyant,* though quite extreme, was nevertheless intrinsically related to the chief concerns of the day. Its glorious Biblical verbiage aside, the tract demanded a truly popular government, a real participation by the masses in the political and economic activities of their society. Much ahead of its time in its democratic message, Lamennais's position represented one meaningful answer to the crucial problem of the July Monarchy, that of the nature of political representation and participation. Romantic adoration of the people blended with the radical legacy of the Enlightenment. Over Lamennais and his contemporaries loomed the huge shadow of the French Revolution.

Lamennais's concept of social progress and the rise of the masses as the fulfillment of the principles of Christianity in history was also a dominant idea of the age. None other than Guizot wrote,

> it is of the nature *par excellence* of the principle and fact of Christianity to have chased away from human thought that iniquity [slavery] and to have extended to entire humanity this right to justice, to sympathy, to freedom, limited until then to a small number and subject to inexorable conditions . . . Jesus Christ came to raise man on earth at the same time that He ransomed him for eternity.
>
> All of Europe, and notably France, has marched for fifteen centuries along the same road of liberation and general progress. This has brought the peoples that have most resolutely undertaken the march to that high degree of power, prosperity and greatness, which we call, and which we have the right to call, modern civilization. This

[48] *Les paroles d'un croyant*, 125.

[49] *Ibid.*, 122. For Lamennais's reasoned defense of property and inheritance in another work of the same period as *Les paroles d'un croyant*, see *Oeuvres complètes*, 11: 170. Indeed, as Lamennais developed his plan for transforming society, he came to regard the possession of property by every member of—or, rather, every family in—society as the foundation of the new order. Especially see *Du passé et de l'avenir du peuple*, published in 1841.

[50] *Les paroles d'un croyant*, 168. ("Mal" can also be translated as "evil" and that would make the point much stronger, but I think that "misfortune" is correct here.) Also see Lamennais's argument, stated especially in his *Affaires de Rome*, that he considered the rise of the masses inevitable and that it was, therefore, very important for the Church—and thus for Lamennais—to guide the masses in the interests of moderation and orderly progress.

[51] Nicholas I hated Louis-Philippe as the usurper of the French throne which rightfully belonged to the senior Bourbon line. After the February Revolution he expressed his satisfaction that the King of the French went out "by way of the same door through which he came in," which, of course, did not mean that the Russian emperor approved of the revolution of 1848, only that he was pleased about the fall of Louis-Philippe. As quoted in A. Lobanov-Rostovsky, *Russia and Europe: 1825–1878* (Ann Arbor, 1954), 116.

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civilization is above all the product of that great idea that every man, for the sole reason that he is man, has the right to justice, to sympathy, to freedom. It was Jesus Christ Who made this idea enter the human soul, whence it has been working to express itself in society.[52]

Curiously, but not inappropriately, Lamennais's tract can, in a sense, be considered a brief and very emotional conclusion to Guizot's many learned volumes. Other French intellectuals could fully understand Lamennais, even when they did not agree with him, and so could the French government, of which Guizot himself was the chief minister and the most prominent spokesman for eight years. There was no official government ideology in France after 1830; Victor Cousin's philosophic eclecticism (which, according to de Vigny, gave, like the moon, light but not warmth) and numerous other teachings served as the intellectual fare of the supporters of the July compromise. By contrast, the Russian government propagated the doctrine of Official Nationality, which emphasized the principles of "Orthodoxy, Autocracy, and Nationality" in the narrow interpretation characteristic of the period of restoration and reaction.[53] Cousin, like Serge Uvarov, the founder of the doctrine of Official Nationality, served as the French minister of education, although Cousin's term of office was much shorter and his impact less significant than the term and impact of his Russian counterpart. Later, Guizot maintained that the July Monarchy was based on freedom of expression and that under Louis-Philippe a rich intellectual dialogue and debate never ceased. Although others might want to qualify the reminiscences of the increasingly conservative and at times repressive prime minister, France was in any case very different from Russia, where Nicholas I proclaimed Chaadaev mad and his head of police, Alexander Benckendorff, could not make any sense of the Slavophiles.[54]

That French intellectual life of the 1830s and 1840s was remarkably unified and cohesive—at least when compared with the Russian experience—raises

[52] Guizot, *Trois générations, 1789–1814–1848* (Paris, 1863), 225, 230. Although he emphasized Christianity, Guizot was essentially a pluralist and an eclectic. Greater stress on the role of Christianity in history could be found in France in, for example, the works of de Bonald and his followers.

[53] On Official Nationality, see my *Nicholas I and Official Nationality in Russia, 1825–1855* (Berkeley and Los Angeles, 1959). The official ideology of Nicholas I's reign was closest in France to the views of the ultras, to whom Nicholas I remained personally devoted until his death. The Revolution of 1830 did much to diminish the importance of such views in France, but in Russia Official Nationality, sponsored by the state, continued to occupy center stage. It can even by said that the Russian government retained much of its archaic ideology up to the revolutions of 1917.

[54] If resemblance in tone is sought between the Russian and the French governments of the time, it can be found in police reports. In his annual report for 1833 Benckendorff wrote to Nicholas I, "the high surveillance, paying searching attention to the disposition of minds, can this time too attest that the devotion and love of his subjects for the sovereign has by no means weakened, and that the noxious urgings and tricks coming from the outside serve only to reveal with a new force the feeling of the people for its tsar"; "Obozrenie raspolozheniia umov i razlichnykh chastei gosudarstvennogo upravleniia v 1833 godu," Tsentralnyi Gosudarstvennyi Arkhiv Oktiabrskoi Revoliutsii, 109/85/1, 198. And the prefect of Lozère stated in 1838 that "nothing has occurred to disturb the happy calm which the department has been enjoying in every respect; our excellent people, with its ingrained habits of religion, order, submission to laws, and respect for officials, continues to demonstrate its attachment to the monarchy." As quoted in S. Charléty, *La Monarchie de Juillet,* vol. 5 of *Histoire de France contemporaine depuis la Révolution jusqu'à la paix de 1919*, ed. Ernest Lavisse (Paris, 1921), 150.

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questions that go beyond the differences between the two countries in political institutions and public policy. Russia experienced two intellectual transformations in the first half of the nineteenth century: a change from the ideology of the Age of Reason to romanticism and philosophic idealism, and the disintegration of those new world views.[55] The abandonment of the philosophy of the Enlightenment shattered intellectual unity in Russia, a unity which had prevailed for a century, in the course of which even such opponents as Alexander Radischev and Catherine the Great or Alexander I and the Decembrists had understood each other perfectly. Indeed, the doctrines of the Age of Reason probably provided the last truly unifying ideology not only for Russia but also for all of Western civilization. When Chaadaev and others introduced the romantic *Weltanschauungen,* the universe of thought was split several ways, both because new ideas penetrated with uneven speed and depth and especially because the ideas themselves no longer offered unity.

Ivan Kireevskii, Khomiakov, and their Slavophile associates and Herzen, Bakunin, Belinskii, and other Westernizers all tried to find their ideological identity in the new intellectual age and to relate their thought to the world in which they lived. The Slavophiles produced the most impressive Russian romantic ideology: coherent, comprehensive, and focused on such central elements of Russian life as Orthodoxy and the peasant commune. They also showed a remarkable tenacity in holding to their views, which, however well developed and bravely championed, led nowhere. The thought of the Slavophiles was too far removed from reality; there could be no return to the Russia they wanted. The Westernizers, on the other hand, especially the radical Westernizers, with all of their tergiversations, tried to stay closer to reality. In so doing they changed, often eventually lost, their beliefs. Yet, although the Slavophiles proved superior to their celebrated opponents as romantic ideologists, the Westernizers better expressed the spirit, the quest, the dynamic of romanticism. The evolution of Westernizer thought might almost convince a sceptic that the idealistic dialectic exists: the Westernizers moved by thesis and antithesis, or at least through contradiction and argument, from Schelling to Hegel, to the Left Hegelians, and beyond. They exhibited best the disintegration of romanticism and idealism. But the defeat of romantic ideology in Russia was not limited to the Slavophile cul-de-sac and the Westernizer catastrophe. Rather, as Belinskii, Granovskii, and the elder Slavophiles died, as Bakunin came to anarchism and a philosophy of destruction, and as Herzen arrived at personalist humanism and despair, idealistic philosphy lost ground, *Naturphilosophie* was abandoned in the natural sciences, and literature turned to realism. In the Russian case, it is remarkable how thorough the ideological collapse was, how clean the boards had been swept for the decade of the 1860s.

Was there an intellectual transformation in France in the first half of the nineteenth century? Certainly there was much important change. More

[55] I dealt with these two transformations in my *A Parting of Ways: Government and the Educated Public in Russia.*

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prominently than his Russian counterparts, Cousin used the ideas of German thinkers to construct pretentious syntheses. French romantic poetry, like Russian romantic poetry, proved one of the glories of the age.[56] French romantic painters, composers, and historians have attracted much-deserved attention. And yet in French culture the tremendous legacy of the French classical age, the Age of Reason, and the French Revolution always remained. It was not that a rich romantic culture did not flower in France. It was rather that somehow that culture, with all its abundance and genius, failed to penetrate and alter the fundamental core. Or, in terms of continuity and change, it is the continuity that is so striking in French literary, intellectual, and cultural history. As the Marquis de Luppé wrote of Lamartine, perhaps the greatest French romantic poet, he was "in 1820 the first of those who have been called romanticists." Judged "by his themes and his music, Lamartine stood alone." He was, moreover, an innovator—"an innovator with a sense of measure, who breaks nothing. His images are without violence, and even without realism. . . . He remained, it has been said, a classicist, a grandson of Racine. . . . By what he rejected and by what he conserved, he kept himself within the confines of the great right way, the French way."[57] Another romantic genius (and one who expressed best in French literature certain extreme romantic positions and attitudes), Alfred de Vigny, has often been called the most classical of the French romantic writers. And where do Victor Hugo and Alfred de Musset belong if not in the middle "of the great right way, the French way"?

Continuity is more striking in political and social thought. Men like Benjamin Constant and Guizot himself spanned generations and political systems. The national debate had been going on well before 1789, since it had been begun and nourished by the philosophes several decades prior to the Revolution. Representation, participation, constitution, the correct political and social system were at the center of attention. *Les paroles d'un croyant* obtained its resonance because it pointedly addressed the main issues, albeit from an exteme position. By contrast, no comparable debate took place in Russia, at least not in concrete political and social terms and not in public.

Lamennais's radical optimism also linked him to the Age of Reason, more especially to its exteme utopian wing. True, his expectation of a virtual palingenesis had a number of sources ranging from the Gospel and the Christian tradition in general to Ballanche and the exalted romantic emotions and perceptions of the time. Still, it was in complete accord with the Enlightenment belief in man's ability to build a reasonable, just, and good—some would say perfect—society. Another remarkable prophet of the epoch,

[56] The recurrent assertion that romanticism somehow did not fit the Russian literary genius and did not produce important results in Russian literature is simply wrong. Thus in poetry, which is in question here, Russia had two supreme romantic poets, Michael Lermontov and Theodore Tiutchev, not to mention others, and romantic works by literary figures—notably Pushkin—who essentially did not belong to romanticism.

[57] Luppé, *Les trauvaux et les jours d'Alphonse de Lamartine*, 86. Lamartine's poetry reminds one more of Pushkin's than of Lermontov's.

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Charles Fourier, drove the maxims of the Age of Reason to absurdity with his design for the phalanx and a regeneration of the world. The visions of Lamennais and Fourier were not the same, but they had much in common, and they appealed to the same moral imperative. As Victor Considérant, Fourier's most prominent disciple, put it as an epigraph in his *Destinées sociales* (in words strikingly reminiscent of Lamennais's *Les paroles d'un croyant*), "All will be called and all will be chosen."[58]

A CONSIDERATION OF Lamennais's *Les paroles d'un croyant* and of Chaadaev's first "Philosophical Letter" in the context of the romantic revolt in France and Russia leads to unsurprising results. The examination reveals once again the power and continuity of the French literary and intellectual tradition, particularly the French classical period and the writings and ideas of the Age of Reason, and sheds some light on the highly developed political life of a France engaged in a whole series of constitutional experiments and centrally concerned with the problem of political participation. Views and attitudes characteristic of the romantic age certainly affected French society, culture, and thought, but these ideas did not fundamentally change the nature of French development. The July Monarchy has usually been considered the apogee of the upper bourgeoisie. No wonder that the literary work which produced the greatest shock in French society and culture was a brilliant tract, a literary classic, which appealed to the masses—in the name of Christ—to rise and win for themselves the self-determination and the fruits of progress which had been monopolized thus far by their betters. The appeal was all the more striking not only because it marked another stage in Lamennais's defiance of the Roman Catholic Church but especially because it was related to an existing radical ideology and movement. Workers and the poor masses in general were in the streets of Lyons and Paris, not merely in the pages of Lamennais's tract.

Things were different in Russia. A newcomer to modern Europe in the early eighteenth century, particularly to modern European intellectual and cultural life, Russia faithfully—and successfully—advanced through the Age of Reason. The fundamental thought of the Enlightenment fitted especially well the historical position of Russia. Still, the resulting culture was bound to be thinner, less organic and more superficial, than the authentic centuries-old French product. Russia thus offered more fertile ground for Chaadaev's nihilism and other far-reaching romantic ideas. Moreover, Russia remained an autocracy based on the gentry and an enserfed peasantry. The middle class

[58] Considérant, *Destinées sociales*, 3 (Paris, 1844): 417. As an epigraph beginning all three volumes of his main work (Paris, 1834–44) and preceded only by an epigraph from Fourier, Considérant placed this passage: "Where are you going, young soldier? I am going to fight for eternal laws descended from on high. . . . May your arms be blessed, young soldier. De La Mennais." These particular lines from *Les paroles d'un croyant* were perhaps the most popular of all.

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was weak and of relatively little intellectual or cultural prominence.[59] And, in general, the life of the spirit in Russia was both much more restricted in content and limited in scope than in France. In the country of the tsars the literary shock and scandal of the age was a paradoxical, abstract, historiosophical declaration of an isolated individual, which asserted the historical nullity of Russia and which served in turn as a starting point for a drawn-out, abstract, historiosophical debate among other Russian intellectuals.

Much in the comparison, then, supports the frequently expressed view that modern Russia received its ideas second- and third-hand and that these ideas tended to become more general and more abstract as they moved eastward. Concrete political debate, notably parliamentary debate, and actual social protest became abstract historical or cultural considerations. Russia transformed events of Western history into morality tales, the lessons of which sometimes did not even then seem to apply. In a sense, Chaadeaev expressed it all when he equated Russian history with zero—and wrote his equation in French.

Yet, historical logic works only up to a point, especially in intellectual history. The apparently superior realism and rationality of French thought can frequently be misleading. Was it really more valid to believe, like so many French intellectuals, that France was everything than to assert, like Chaadaev, that Russia was nothing? How is one to account for the salient militarism of the French intellectual climate of the age? It would seem that Louis-Philippe received his greatest support in the Chamber of Deputies and in public opinion when he was willing to oppose Great Britain.[60] Fortunately, the stubborn king realized that a war against the British was the last thing the country needed. Lamennais, it might be added, was among the militarists, though of a different kind. He championed war or, if needed, a whole series of wars in a Holy Alliance of peoples against their oppressors. Other similarly disturbing issues readily come to mind. Indeed, the ultimate interest of Lamennais's *Les paroles d'un croyant* and Chaadaev's first "Philosophical Letter" lies in their magnificent presentation of ideas and obsessions that move men and, through men, history.

[59] For example, P. G. Ryndziunskii, one of the leading specialists on the Russian middle class, has pointed out that the merchants "did not manifest themselves in the abolitionist movement, and, therefore, their social views are not reflected in the materials illuminating that movement"; *Gorodskoe grazhdanstvo doreformennoi Rossii* (Moscow, 1958), 86. The contrast with Fourier and other French intellectuals who came from merchant families is readily apparent.

[60] See, for example, Charléty, *La Monarchie de Juillet*, 313.

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Witchcraft Trials in Seventeenth-Century Russia

RUSSELL ZGUTA

In his controversial essay "The European Witch-Craze of the Sixteenth and Seventeenth Centuries," H. R. Trevor-Roper noted that, as a result of the Schism of 1054, "the Slavonic countries of Europe—with the exception of Catholic Poland, the exception which proves the rule—escaped participation in one of the most disreputable episodes in Christian history." Similarly, in his recent book *Europe's Inner Demons* Norman Cohn observed that the great witch hunt was "an exclusively Western phenomenon—Eastern Europe, the world of Orthodox Christianity was untouched by it."[1] Did Muscovite Russia, that proverbially "rude and barbarous kingdom," indeed manage to preserve its innocence while other, ostensibly more civilized nations went mad with witch hysteria? At least one highly respected Russian scholar, Nikolai Ia. Novombergskii, did not think so. In the introduction to his collection of seventeenth-century Muscovite witchcraft trial records published in 1906, he wrote, "Considering the overall significance of the historical documents presented here, we find that witchcraft trials were conducted in Russia with the same degree of cruelty as those in the West and that government authorities of the Muscovite state cooperated in these trials as zealously as did their counterparts in Catholic and Protestant countries."[2]

Keith Thomas has recently defined witchcraft "as the attribution of misfortune to occult human agency." According to this definition, a witch was someone with the ability to injure others mysteriously. The terms *vedun*

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[1] Trevor-Roper, "The European Witch-Craze of the Sixteenth and Seventeenth Centuries," in *Religion, the Reformation, and Social Change* (London, 1967), 185. In an accompanying footnote (note 2) he elaborated briefly on this statement by remarking that "the one great Church Father who wrote before the schism and who provided an intellectual basis for the later witch-craze, St. Augustine, had little or no influence in Byzantium. This was no doubt partly because he wrote in Latin. Without Augustine, without Aquinas, the Greek Church lacked the cosmological infrastructure of the witch-craze." And Cohn, *Europe's Inner Demons: An Enquiry Inspired by the Great Witch-Hunt* (New York, 1975), 253.

[2] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka* (St. Petersburg, 1906), xxxii. A similar opinion has been expressed by E. Artsikhovskaia, who views the Russian witch trials as differing only in number from those in the West at the height of the witch craze; see her *O koldovstve, porche i klikushestve* (St. Petersburg, 1905), 10.

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(warlock) and *ved'ma* (witch) are common in seventeenth-century Russian sources, while "witchcraft" is frequently rendered as *koldovstvo* or *charodeistvo*. Earlier references to what may be construed as witches and witchcraft cover a broad range of occult practices including magic, spell-casting, fortunetelling, divination, dream interpretation, weather manipulation, and herb medicine. Basically, however, in the eyes of the people a witch was a person of either sex who could mysteriously injure another. The damage a witch might do was technically called *maleficium* in the West. The Russian equivalent of *maleficium* was *porcha*, a term which in Old Russian denoted "damage" or "injury" of any sort.[3]

Although Russian scholars have demonstrated some interest in various aspects of sorcery, magic, and witchcraft among the people,[4] no one has, to date, attempted a systematic study of the important legal documents first brought to light by Novombergskii some seventy years ago. Novombergskii stumbled upon these documents quite by accident while working in the Moscow Archives of the Ministry of Justice (*Moskovskii Arkhiv Ministerstva Iustitsii*) searching for material related to the history of pre-Petrine medicine. He subsequently published some of them in a separate volume devoted to witchcraft in seventeenth-century Russia.[5] The remainder appear scattered throughout the appendixes of his two works on early Russian medicine.[6] Although many of these official trial transcripts are incomplete (they frequently lack a final verdict, for example), they nonetheless provide a veritable gold mine for the student of Muscovite history, ethnography, criminal law, and medicine. Chronologically, they span the period from 1622 to 1700 and thus coincide with the waning days of the witch craze in the West.

PRIOR TO THE SEVENTEENTH CENTURY, Kievan and Muscovite sources record earlier instances of witch persecution. The first such references occur in the *Primary Chronicle*: as early as 1024 several old people were executed in Suzdal after being blamed for the food shortage which had resulted from a serious

[3] For a very brief, general treatment of Russian witchcraft, see W. R. S. Ralston, *The Songs of the Russian People* (London, 1872), 415–34. Howard Charles Lea, in his otherwise comprehensive three-volume collection of *Materials Toward a History of Witchcraft*, ed. Arthur C. Howland (New York, 1957), does not venture farther east than Poland. For the terminology used here, see I. I. Sreznevskii, *Materialy dlia slovaria drevnerusskogo iazyka*, 2 (Graz, 1956): 1756. And for Thomas's definition and general description of the phenomenon, see his *Religion and the Decline of Magic* (New York, 1971), 436.

[4] For a basic bibliography of Russian works on witchcraft and related topics, see N. L. Brodskii *et al.*, eds., *Russkaia ustnaia slovesnost'* (Leningrad, 1924), 16–17. Other works, not found in the above bibliography, include A. A. Levenstim, *Sueverie i ugolovnoe pravo* (2d ed.; St. Petersburg, 1899), and "Sueverie i ugolovnoe pravo," *Vestnik prava*, pt. 1, 1 (1906): 291–343, and pt. 2, 2 (1906): 181–251; Artsikhovskaia, *O koldovstve, porche i klikushestve*; Elena Eleonskaia, "Zagovor i koldovstvo na Rusi v XVII i XVIII stoletiiakh," *Russkii arkhiv*, 50 (1912): 611–24; G. Esipov, "Koldovstvo v XVII i XVIII stoletiiakh," *Drevniaia i novaia Rossiia*, 4 (1878): 64–70 and 157–64; L. V. Cherepnin, "Iz istorii drevne-russkogo koldovstva XVII v.," *Etnografiia*, 2 (1929): 86–109; and Dmitrii Zelenin, *Russische (Ostslavische) Volkskunde* (Berlin-Leipzig, 1927), 394–99.

[5] Although published separately, these documents also appeared under a slightly different title—*Koldovstvo v Moskovskoi Rusi XVII stoletiia*—as volume 4, part 1, of the *Materialy po istorii meditsiny v Rossii* (St. Petersburg, 1906).

[6] Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi* (Tomsk, 1907), Prilozhenie, nos. 5, 7, 10–11, 46–47, 51, and *Materialy po istorii meditsiny v Rossii*, 4: nos. 16–20, 28, 35, 37–41, 44, 47, 55, 60, 151, 155, 171–72.

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drought. A similar incident took place in Rostov in 1071 where a number of women, both young and old, were put to death in connection with a famine.[7] The relationship between natural calamity and periodic witch hunts in medieval Muscovy became even more pronounced in an unusual sermon of Serapion, the thirteenth-century bishop of Vladimir. At a time when Russia was being devastated by a famine which lasted from 1271 until 1274, Serapion upbraided his countrymen for their lingering paganism, especially their belief in witchcraft. He also excoriated them for their informal trials and summary executions of women accused of causing drought through witchcraft.[8]

In addition to providing a clue to the underlying pattern which characterized these earliest recorded instances of witch hunting in Muscovy, Serapion's sermon contains two other revealing pieces of information: burning was the usual punishment meted out for witchcraft and, perhaps even more significantly, the customary method of establishing guilt or innocence in cases of alleged witchcraft was the ordeal by cold water (*judicium aquae frigidae*), popularly known in the West as "swimming a witch." Both of these practices, so intimately identified with witch hunting in sixteenth- and seventeenth-century Europe, find further confirmation in foreign and native medieval sources. Abu Hāmid al-Gharnātī (1080 or 1081–1169 or 1170), the Arabic traveler-merchant who visited Kiev in 1153, left an intriguing account of a multiple witch swimming and burning which he either witnessed or heard about:

> Every twenty years the old women of this country become guilty [suspected?] of witchcraft, which causes great concern among the people. They then seize all of those [women] they find in this area and throw them feet and fists tied [together], into a big river [Dnieper?] that passes through there. Those who stay afloat are considered to be witches and are burned; those who, on the contrary, go under are declared innocent of all witchcraft and are set free again.[9]

Both the Novgorod and Pskov chronicles record multiple witch burnings. In Novgorod four male witches were burned in 1227 for allegedly "practicing sorcery"; in Pskov twelve female witches were burned in 1411.[10] Although the chronicler offered no explanation for this mass auto-da-fé, we do know from

[7] Dmitrij Tschižewskij, *Die Nestor-Chronik* (Wiesbaden, 1969), 144, 170; and Samuel H. Cross and Olgerd P. Sherbowitz-Wetzor, eds. and trans., *The Russian Primary Chronicle* (Cambridge, Mass., 1953), 134, 151–52.

[8] Evgenii V. Petukhov, *Serapion Vladimirskii, russkii propovednik XIII veka* in *Zapiski istoriko-filologicheskago fakul'teta Imperatorskago S.-Peterburgskago universiteta*, 17 (1888): 1–13 Appendix (full text of sermon), 63–65 (commentary on text). Serapion had been abbot of the Pecherskii monastery in Kiev. He was made bishop of Vladimir in 1274 and died a year later. *Ibid.*, 1. Also see Arcadius Kahan, "Natural Calamities and Their Effect upon the Food Supply in Russia (An Introduction to a Catalogue)," *Jahrbücher für Geschichte Osteuropas*, 16 (1968): 368.

[9] M. Charmoy, "Relation de Mas'oudy et d'autres musulmans sur les anciens Slaves," *Mémoires de l'Académie des Sciences de S. Petersbourg*, 6th ser., 2 (1834): 342–43. For Abu Hāmid's visit, see César E. Dubler, *Abu Hāmid el Granadino y su relación de viaje por tierras eurasiaticas* (Madrid, 1953), 129; also see the map following page 132. Another source—*Ukrajins'ka radians'ka entsiklopediia*, 1 (Kiev, 1959): 17—maintained that Abu Hāmid actually visited Kiev on three separate occasions between 1150 and 1153 and that he was received by Prince Iziaslav Mstislavovich.

[10] A. N. Nasonov, ed., *Novgorodskaia pervaia letopis' starshego i mladshego izvodov* (Moscow-Leningrad, 1950), 65; Robert Michell and Nevill Forbes, trans., *The Chronicle of Novgorod, 1016–1471* (London, 1914), 69; and A. N. Nasonov, ed., *Pskovskie letopisi*, 2 (Moscow, 1949): 36.

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other sources that a serious plague decimated many parts of Muscovy, including Pskov, from 1410 through the 1420s.[11] This natural calamity might explain the Pskov burnings; it is also possible, however, that the executions were a direct response to Metropolitan Fotii's letter of 1410 urging the people of Novgorod and vicinity to rid themselves of all vestiges of paganism, including witchcraft.[12] Burning at the stake continued to be one of the punishments prescribed for witchcraft—the other was exile—even in the early eighteenth century. But ordeal by water as a test for witchcraft seems to have fallen into desuetude quite early in the Muscovite lands, never to be revived again. Farther west, however, among the Ukrainians of the former Kievan lands, women accused of witchcraft were routinely tested by swimming well into the late nineteenth century.[13]

These practices were the popular, extralegal responses to witchcraft in Kievan Rus' and Muscovite Russia. Legally witchcraft came, very early, under the jurisdiction of the Church, where it largely remained well into the eighteenth century. The first edition of Vladimir the Great's Church Statute (*Ustav*), dating from the second half of the eleventh century, enumerated the crimes subject to trial in the ecclesiastical courts. Among these were witchcraft and the use of spells, talismans, and potions.[14] While Vladimir's *ustav* defined the area of jurisdiction for the ecclesiastical courts, the Byzantine *Nomokanon* (known as the *Kormchaia kniga* in the Slavonic version) was intended, according to most scholars, to serve as the operative legal code governing certain aspects of civil and penal law as well as canon law. The *Nomokanon* prescribed death by the sword as punishment for witchcraft.[15] Vladimir and his successors, however, began to substitute a system of fines for the more draconian penalties called for by the *Nomokanon*.[16] Thus, the expanded version of the Church Statute of Iaroslav the Wise, dating from the late twelfth or early thirteenth century, stipulated the amount of fine to be paid by a woman accused of practicing the black arts: "If a woman be a sorceress or a purveyor of talismans or a witch or an herbalist, and her husband rebuke her, and if she does not cease she must pay six *grivna* to the Metropolitan." By comparison, incest between brother and sister was punish-

[11] Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, 17–18.

[12] "Poslanie mitropolita Fotiia v Novgorod o sobliudenii zakonopolozhenii tserkovnykh," *Russkaia istoricheskaia biblioteka*, 6 (St. Petersburg, 1880): 274.

[13] In 1731, during the reign of the Empress Anna, the Senate formally recommended burning as a punishment for witchcraft. *Polnoe sobranie zakonov Rossiiskoi imperii* (hereafter *PSZ*), 1st ser., 8 (St. Petersburg, 1830): no. 5761. Cf. Volodymyr Hnatiuk, "Kupanje'j palenje vid'm u Halychyni," *Materialy do ukrajns'koji etnol'ogiji*, 15 (1912): 178–201. Also see my "The Ordeal by Water (Swimming of Witches) in the East Slavic World," *Slavic Review*, 36 (1977): 220–30.

[14] S. V. Iushkov, ed., *Pamiatniki russkogo prava*, vol. 1: *Pamiatniki prava kievskogo gosudarstva X–XII vv.*, ed. A. A. Zimin (Moscow, 1952), 238 (article 7). A later, third edition, of this same *ustav* (page 245 [article 6]) is even more detailed in its enumeration of the "occult" crimes relegated to the ecclesiastical courts: fortunetelling, quack medicine, witchcraft, magic, sorcery, casting of spells, and the use of potions. On the dating of the various early Church statutes, see Ia. N. Shchapov, *Kniazheskie ustavy i tserkov' v drevnei Rusi XI–XIV vv.* (Moscow, 1972).

[15] Iushkov, *Pamiatniki russkogo prava*, 1: 237 (article 4); also see Levenstim, "Sueverie i ugolovnoe pravo," pt. 1, 308–09.

[16] Levenstim, "Sueverie i ugolovnoe pravo," pt. 1, 309.

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The Witch Baba Iaga and the Crocodile Demon. Woodcut, possibly by Vasilii Koren. Early eighteenth century.

able by forty *grivna*, and a wife who beat her husband merited a fine of three *grivna*.[17]

By the mid-sixteenth century both the Muscovite Church and state began to show greater concern for surviving manifestations of paganism—including witchcraft—among the people. Some of the recommendations and decisions of the important *Stoglav* Church Council of 1551 reflect this concern most vividly. The tsar himself, Ivan IV, put the question of witchcraft before the Church fathers. He asked, first of all, what should be done with those who practiced the black arts, such as divination, fortunetelling, and astrology. The council responded that the tsar should impose the death penalty on and the bishops should excommunicate these evildoers.[18] In a related question, the tsar asked the council fathers for their opinions regarding litigants who invoked "God's justice" as part of their trial procedure and then—before they actually kissed the cross or icon or went forth into the dueling field—hastened to consult a sorcerer about the outcome of the trial. In reply, the council urged the sovereign to outlaw sorcerers and, whenever caught, punish them severely. The Church, for its part, must continue to discourage the faithful from

[17] Iushkov, *Pamiatniki russkogo prava*, 1, 268–69 (articles 37, 13, and 39).
[18] *Stoglav*, chap. 41, question 22; and E. Duchesne, trans., *Le Stoglav, ou les cent chapitres* (Paris, 1920), 122.

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believing in sorcery and witchcraft and must excommunicate the recalcitrant.[19] In a special *ukaz* of 1552, which ordered full compliance with the council's decisions, the tsar warned all his subjects—particularly those about to engage in a judicial duel—to shun sorcerers, fortunetellers, and astrologers or risk the tsar's wrath and prosecution according to both the civil laws of the land and the ecclesiastical canons of the Church.[20] Much to his credit, Ivan IV heeded only part of the advice given him by the *Stoglav* fathers on the question of witchcraft: the council's other recommendation, that the state formally punish witches with death, went unheeded. But Ivan's reference in his *ukaz* of 1552 to civil prosecution for witchcraft is of considerable importance, for it provided a legal basis for the eventual trial of witches in civil as well as ecclesiastical courts. By granting the civil courts jurisdiction over witchcraft, the tsar transformed what had hitherto been regarded as a residual manifestation of paganism among the people into a criminal offense,[21] so that, in a very real sense, he helped to pave the way for the witch trials of the seventeenth century.

In addition to the *Stoglav* decisions of 1551, other signs indicated the increasing concern of both Church and state about witchcraft. The Penitentials of the fifteenth and sixteenth centuries show a marked increase in the number and variety of sins related to witchcraft for which penitents—both male and female, lay and religious—were held accountable.[22] The Muscovite court seemed more and more preoccupied with witchcraft and sorcery, and fear that some evil witch or warlock would do harm to the tsar and his family was ever present. As soon as the tsar or any member of his immediate family became ill, it was generally assumed that the illness was caused by witchcraft. The tsar's children were assiduously guarded from the gaze of outsiders lest they become victims of the evil eye. Ivan III claimed that his first wife, Maria of Tver, had been bewitched to death in 1467. And in 1497 Ivan became angry at his wife, Sofiia, because she was allegedly being visited by witches who, he suspected, might provide her with potions to poison him. He ordered the evil women seized and drowned in the Moscow River in the dead of night.[23] The son of Ivan III, Vasilii III, insisted that all officials renounce "evil potions" when they swore their customary oath of allegiance to the new tsar.[24]

The reign of Ivan IV not only provides a graphic example of witch paranoia

[19] *Stoglav*, chap. 41, question 17; and Duchesne, *Le Stoglav*, 118. For an excellent description of "God's justice" and its place in Muscovite trial procedure, see Ann M. Kleimola, "Justice in Medieval Russia: Muscovite Judgment Charters (Pravye Gramoty) of the Fifteenth and Sixteenth Centuries," *Transactions of the American Philosophical Society*, new ser., 65, pt. 6 (1975): 58–68.

[20] *Akty istoricheskie, sobrannye i izdannye Arkheograficheskoiu Komissieiu*, vol. 1: *1334–1598* (St. Petersburg, 1841), nos. 154, 252.

[21] M. F. Vladimirskii-Budanov, *Obzor istorii russkago prava* (7th ed.; Petrograd-Kiev, 1915), 343. On secular punishment for ecclesiastical crimes, see Nikolai S. Suvorov, *O tserkovnykh nakazaniiakh* (St. Petersburg, 1876), chap. 4.

[22] A. Almazov, *Tainaia ispoved v pravoslavnoi vostochnoi tserkvi*, vol. 1, in *Zapiski Imperatorskago novorossiiskago universiteta*, 63 (Odessa, 1894), 406–09, and also Appendix to vol. 1, in *Zapiski Imperatorskago novorossiiskago universiteta*, 65 (Odessa, 1895): 152, 161–62, 164–69, 172, 177, 179–80, 183–84.

[23] *Polnoe sobranie russkikh letopisei* (hereafter *PSRL*), vol. 6: *Sofiiskiia letopisi* (St. Petersburg, 1853), 186, 279.

[24] Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, 13.

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at court but also helps illustrate the use of witchcraft for political purposes. In 1547, the year Ivan formally ascended the throne, a fire devastated Moscow and engendered widespread popular unrest.[25] A rumor, circulated by Ivan's closest advisers, alleged that Ivan's grandmother, Anna Glinskaia, and her children produced the catastrophe through sorcery.[26] The enraged, frustrated mob needed no further prodding. It sought out and punished the Glinskiis and thus rid Ivan of his political opponents and smoothed the way for his consolidation of power.

In 1549 Ivan S. Peresvetov, an ardent spokesman on behalf of autocracy and strong central government, sent a petition to the tsar which warned him to beware of witches and magicians whom his opponents might use to bring him down.[27] In addition, in the mid-sixteenth century at least one widely circulated tale portrayed Ivan's closest advisers (presumably the priest Sil'vester and Aleksei Adashev) as warlocks.[28] According to Peter Sadikov, these ominous warnings of impending evil must have had considerable influence on Ivan who, as a true son of his age, firmly believed in witchcraft.[29] He was fairly convinced, for example, that sorcery caused the death of his beloved wife, Anastasiia, in 1560.[30] During the *oprichnina* purges (1565–72), Ivan levelled the charge of witchcraft against several of the boyars whom he wished to liquidate.[31]

During the Time of Troubles (1598–1613) fear of witchcraft increased markedly among both the legitimate and not so legitimate Russian rulers. According to Sergei M. Solov'ev, Boris Godunov falsely accused the Romanovs of using witchcraft to try to wrest the throne from him in 1601. For this he had the entire family exiled.[32] Both Godunov and Vasilii Shuiskii insisted that the oath of allegiance now include lengthy renunciations of witchcraft and a host of other occult practices which might be used to harm the tsar and his family.[33] A *gramota* issued in 1605 by the first False Dmitrii, instructing the

[25] For a lengthy discussion of the fire and its aftermath, see S. O. Shmidt, *Stanovlenie Rossiiskogo samoderzhavstva* (Moscow, 1973), 13–119.

[26] According to the chronicle, she allegedly removed human hearts, steeped them in water, and then sprinkled them on the streets of Moscow, thus causing the fire. *PSRL*, vol. 13, pt. 2: *Tak nazyvaemaia tsarstvennaia kniga* (St. Petersburg, 1906), 456. Also see Bjarne Nørretranders, *The Shaping of Czardom under Ivan Groznyj* (London, 1971), 94–97; and I. I. Smirnov, *Ocherki politicheskoi istorii russkogo gosudarstva 30–50 kh godov XVI veka* (Moscow-Leningrad, 1958), 122–27.

[27] V. F. Rzhiga, "I. S. Peresvetov—publitsist XVI veka," *Chteniia v Imperatorskom obshchestve istorii i drevnostei rossiiskikh pri moskovskom universitete*, 1, pt. 2 (1908): materialy istoriko-literaturnye, 59–69.

[28] "Povest' o volkhvovanii, napisannaia dlia tsaria Ivana Vasil'evicha Groznago," *Moskvitianin*, 1 (1844): 246–49. Also see Nørretranders, *The Shaping of Czardom*, 76–80. Another, much shorter tale, preserved in oral form and not recorded until the late nineteenth century, describes Ivan's attempt to burn some witches in Moscow; they, however, turned into crows and flew away. N. Ia. Aristov, "Russkiia narodnyia predaniia ob istoricheskikh litsakh i sobytiiakh," *Trudy tret'iago arkheologicheskago s"ezda v Rossii*, 1 (Kiev, 1878): 339–40.

[29] P. A. Sadikov, *Ocherki po istorii oprichniny* (Moscow-Leningrad, 1950), 9. Also see Shmidt, *Stanovlenie Rossiiskogo samoderzhavstva*, 33.

[30] Nørretranders, *The Shaping of Czardom*, 134.

[31] J. L. I. Fennell, ed. and trans., *Prince A. M. Kurbsky's History of Ivan IV* (Cambridge, 1965), 196–99. Also see S. B. Veselovskii, "Poslednie udely v severo-vostochnoi Rusi," *Istoricheskie zapiski*, 22 (1947): 114; and A. A. Zimin, *Oprichnina Ivana Groznogo* (Moscow, 1964), 97.

[32] Solov'ev, *Istoriia Rossii s drevneishikh vremen*, 4 (Moscow, 1966): 394–95.

[33] *Akty, sobrannye v bibliotekakh i arkhivakh Rossiiskoi Imperii arkheograficheskoiu ekspeditsieiu Imperatorskoi Akademii Nauk*, vol. 2: *1598–1613* (St. Petersburg, 1836), 58–59, 102. Also see A. M. Kleimola, "The Duty to Denounce in Muscovite Russia," *Slavic Review*, 31 (1972): 771–72.

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people of Sol'vychegodsk to swear allegiance to the pretender, also contained a clause dealing with witchcraft.[34]

Even after the turbulence and anarchy of the Time of Troubles had passed and relative peace and legitimacy had been restored, the preoccupation of official Muscovite circles with witchcraft did not abate. In January 1632 Michael Romanov ordered authorities in Pskov to forbid local merchants, under penalty of death, from buying Lithuanian hops because they had supposedly been contaminated by witchcraft. Russian spies operating in Lithuania—no doubt in preparation for the war with Poland that began in December 1632—alleged that "in the Lithuanian towns there are witches who cast spells on hops which are to be brought into our towns so that by means of these hops they can spread plague among the people."[35]

According to the chronicles, Michael Romanov's first wife, who died in 1625, was the victim of malefic witchcraft.[36] Evidently, Michael himself believed it, judging from the three lengthy investigations into suspected witchcraft at court during his reign. In 1635 he ordered the first such inquest after one of the female attendants at court accidentally dropped a handkerchief with an herb in it. After several torture sessions, the unfortunate woman admitted that she had acquired the herb from a witch with the sole purpose of using it as a love potion to make her husband love her better. Her confession did not, however, mitigate her sentence, for she and her husband were both exiled to Kazan while the witch and her husband were exiled to Charonda.[37] An even lengthier and more extensive investigation developed in 1638 after two court seamstresses came to the tsar and informed him that one of their number, a certain Dar'ia Lamanova, had been seen sprinkling ashes over the staircase frequently used by the *tsaritsa*—a common form of witchcraft in the seventeenth century. After the interrogation of numerous witnesses, endless torture, and not a few confessions of guilt, a number of men and women and some entire families were exiled to Siberia and other distant places.[38] A third such inquest took place in 1642 and 1643 after a warlock by the name of Afon'ka had boasted, while in prison, that he intended to bewitch Michael's wife, Evdokiia.[39]

Grigorii Kotoshikhin, a usually reliable source, reported that Tsar Aleksei's first bride-to-be was said to have been poisoned by a witch.[40] It comes as no surprise, therefore, that in 1647, two years into his reign, Aleksei ordered an

[34] *Akty, sobrannye v bibliotekakh i arkhivakh Rossiiskoi Imperii*, 2: 94.

[35] *Ibid.*, 283.

[36] *PSRL*, vol. 31: *Letopistsy poslednei chetverti XVII v.* (Moscow, 1968), 159.

[37] I. E. Zabelin, "Sysknyia dela o vorozheiakh i koldun' iakh pri tsare Mikhaile Fedoroviche," in *Kometa: ucheno-literaturnyi al'manakh,* ed. N. Shchepkin (Moscow, 1851), 477–81.

[38] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka,* 112–34. What the woman actually wanted to do was to sprinkle ashes over the footsteps of the *tsaritsa,* which in this case was impossible since the staircase was of stone. She therefore tried to sprinkle the entire staircase with the ashes in hopes of achieving the same desired effect of making the *tsaritsa* more favorably disposed toward her.

[39] A. N. Zertsalov, *K materialam o vorozhbe v drevnei Rusi: Sysknoe delo 1642–1643 gg. o namerenii isportit' tsaritsu Evdokiiu Luk'ianovnu* (Moscow, 1895).

[40] Kotoshikhin, *O Rossii, v tsarstvovanie Alekseia Mikhailovicha* (2d ed.; St. Petersburg, 1859), 4–5.

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investigation similar to those conducted by his predecessor.[41] The last and perhaps most celebrated case of alleged witchcraft at court took place in 1689. A high ranking serviceman, Andrei Bezobrazov, was accused of procuring the services of certain warlocks to soften the heart of the tsar, Peter I, and keep him from sending the aging soldier to serve in an undesirable, distant post.[42] A high level inquiry found Bezobrazov guilty of endangering the tsar's life by means of the black arts, and a boyar jury sentenced him to death. On January 8, 1690 he was beheaded in Red Square, and the two warlocks whom he had retained were burned at the stake. Bezobrazov's wife, who knew of her husband's plan but failed to report it to the authorities, was ordered to take the veil while a number of other individuals—directly or indirectly implicated—and their families suffered exile to Siberia.[43]

In addition to launching frequent investigations of witchcraft in their own households, seventeenth-century Muscovite rulers occasionally sought to protect their subjects from the same danger. In 1648 Tsar Aleksei issued a decree, circulated throughout the country, in which he condemned a variety of pagan practices, including witchcraft. He ordered the knouting of those caught consulting witches or warlocks for any purpose and made third and fourth offenders subject to exile.[44] In 1653 Aleksei focused an even sterner decree exclusively on the black arts and sent it out to numerous towns and provincial centers. In it the tsar condemned such things as roots and potions, divining dice, manuals of fortunetelling, written incantations, and, of course, the witches and warlocks themselves, who, according to Aleksei, caused much of the suffering and death among the people. He decreed the immediate burning of the "heretical" items enumerated and declared that anyone henceforth caught using them or engaging in malefic witchcraft of any sort would be shown no mercy. The guilty were to be burned at the stake and their dwellings were to be razed.[45] The last such official expression of concern over witchcraft appeared in the founding charter of the Moscow Academy (Slavonic-Greek-Latin Academy), issued by Tsar Fedor III in 1682: article 14 prescribed burning for those guilty of witchcraft, whether teachers or students.[46] But none of these official decrees was intended to be, nor did any of them become, a basis for an organized and systematic persecution of witches.

The foregoing has sketched Russian witchcraft from the earliest times and has demonstrated that the witch trials of the seventeenth century were not a historical aberration. The ground had been well prepared in advance, making the trials not only possible but, indeed, almost inevitable.

[41] Esipov, "Koldovstvo v XVII i XVIII stoletiiakh," 64–66.

[42] For a brief account of the Bezobrazov affair, see Askalon Truvorov, "Volkhvy i vorozhei na Rusi, v kontse XVII veka." *Istoricheskii vestnik*, 36 (1889): 701–13.

[43] A. N. Truvorov, ed., *Rozyskniya dela o Fedore Shaklovitom i ego soobshchnikakh*, 2 (St. Petersburg, 1885): 1–32, 275–316, contains an official record of the inquiry, including a description of the punishment meted out to the guilty.

[44] P. Ivanov, *Opisanie gosudarstvennago arkhiva starykh del* (Moscow, 1850), 296–99.

[45] Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, lxxxiv–lxxxvi; and Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 78–80.

[46] Nikolai Novikov, ed., *Drevniaia rossiiskaia vivliofika*, 6 (2d ed.; Moscow, 1788): 415–17.

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BETWEEN 1622 AND 1700 SOME NINETY-NINE PERSONS were brought to trial for witchcraft in the Muscovite courts. The records of these forty-seven trials, some only fragmentary, have survived largely because the criminal nature of the cases forced their formal referral to higher authorities in Moscow, where they eventually found their way into the archives of the Ministry of Justice. To be sure, other cases of alleged witchcraft were brought before local magistrates which, for one reason or another, did not reach Moscow on *doklad*,[47] as evidence from widely scattered sources confirms. In 1606 an outbreak of hiccups in Perm was attributed to witchcraft. A number of people were tried in connection with this "natural calamity."[48] An archpriest came to trial in the Siberian town of Verkhotursk in 1625 for possession of suspicious roots and herbs.[49] Tsar Aleksei granted the people of Shuia permission to burn a convicted warlock in 1669.[50] In 1671 a witch was convicted and burned in Temnikovo.[51] The total number of such local trials will perhaps never be known. It is reasonably certain, however, that the number of people ultimately implicated in and executed for witchcraft in Muscovite Russia did not approximate that in Western Europe at the height of the witch craze.

Geographically, the Russian trials for which the records survive were not restricted to any one locale. Six were recorded in Moscow, four in Belgorod, three in Efremov, and two each in Kostroma, Voronezh, Lukh, and Nizhnii Novgorod. The rest occurred in various provincial centers—including Suzdal, Mozhaisk, Murom, Iaroslavl, Vologda, Velikie Luki, and others. Although most of the trials were small (no more than a dozen principals and witnesses), at least two had close to fifty witnesses, three included a hundred or more, and one involved 1452 written and oral depositions. Of the ninety-nine accused, forty were women and fifty-nine were men. No children took part, either as principals or witnesses, in any of the trials. Among the accused, the preponderance of men over women and the absence of children set these trials apart from those in the West. And of the ninety-nine, twenty-one were acquitted and released, ten were sentenced to be burned at the stake, five were exiled, three died during torture and interrogation, one jumped bail and fled, and one—a priest—was handed over to the ecclesiastical authorities. The fate of the remaining fifty-eight remains unknown because of the fragmentary nature of the documents and their occasional failure to record a verdict and sentence. In terms of socioeconomic background, neither the accused nor their accusers conform to any rigid pattern. More than half of those accused

[47] The Russian legal term for this procedural step of referring a case to a higher court is *doklad*. Basically a continuation of the original trial, with a re-examination of the evidence by the higher court, the procedure did not entail appeal or review, since no decision had been handed down by the lower court. Also see Kleimola, "Justice in Medieval Russia," 68–74.

[48] *Akty istoricheskie*, vol. 2: *1598–1613* (St. Petersburg, 1841), 82–83.

[49] *Ibid.*, vol. 3: *1613–1645* (St. Petersburg, 1841), 224–25.

[50] Vladimir Borisov, ed., *Starinnye akty, sluzhashchie preimushchestvenno dopolneniem k opisaniiu g. Shui i ego okrestnostei* (Moscow, 1853), no. 109.

[51] A. Popov, ed., *Materialy dlia istorii vozmushcheniia Stenki Razina* (Moscow, 1857), 107–08. S. M. Solov'ev, in *Istoriia Rossii s drevneishikh vremen*, 7: 132–35, has noted some additional seventeenth-century trials involving witchcraft. Also see *Akty, sobrannye v bibliotekakh i arkhivakh Rossiiskoi Imperii*, vol. 4: *1645–1700* (St. Petersburg, 1836), 31.

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were peasants; among the rest were a former military governor, a Cossack captain, a priest, two sextons, two tavern-keepers, three members of the middle service class (*deti boiarskie*), and thirteen transients or foreigners (six Mordvinians or Finns, two Circassians, one Pole, one Tatar).[52] Excluding the fourteen instances in which trials were initiated by the *guba*[53] (or other authorities) or at the behest of entire towns and communities, among the accusers there were peasants, high-ranking noblemen (a *boyarin* and a *kniaz'*), members of the middle service class, a high-ranking serviceman (*stol'nik*), military men, burghers, priests, and even one nun. Five of the accused were related to their accusers, three as husband and wife.

From a legal standpoint, the trial records are particularly revealing, since they reflect the whole range of seventeenth-century Muscovite criminal procedure. Much like those in the West, some of the trials were initiated by local authorities who invoked the inquisitorial method of procedure. In Russia the *guba* also had the power to initiate legal action according to the inquisitorial method and did so with greater frequency after Tsar Aleksei charged them in 1648 and again in 1653 to spare no effort in extirpating paganism, including witchcraft, at all levels of society. Under inquisitorial procedure, local officials could initiate a trial without a private plea or accusation in any one of three ways: on the apprehension of a criminal in the act, on the verdict of an inquest (*poval'nyi obysk*) which had determined the reputation of the accused, or on *ogovor* (torture of the accused until he was forced to incriminate accomplices). In the cases here, the *guba* or *voevoda* used apprehension in the act (or in possession of incriminating evidence—such as herbs, roots, potions, or written incantations) and *ogovor* to initiate witchcraft trials under inquisitorial procedures. Trials initiated by accusatory procedure occurred more often. An individual or group of plaintiffs brought a formal written or oral complaint and presented it to the *voevoda* or some other appropriate official in the nearest provincial office (*s"ezzhaia* or *prikaznaia izba*), which was also used for the administration of justice. The complaint usually took the form of a petition (*chelobit'* or *chelobitnaia gramota*) addressed to the tsar himself. In it the supplicant identified himself by name and profession or social status followed by a statement of the charge or grievance for which he sought redress.[54] At this point the *voevoda* would either begin the trial or would submit the petition to

[52] Also among the accused were four military men of low rank, five wives of military men of various ranks, and one *posadskii chelovek* (burgher).

[53] The *guba* can be defined as a bandit-prosecution district. Beginning in the 1540s special charters (*gubnye gramoty* or *gubnye nakazy*) empowered local communities to seek out and prosecute on their own initiative all notorious local criminals. Originally the competence of the *guba* was restricted to the prevention and suppression of banditry. Soon, however, they received jurisdiction over the closely related crimes of theft and murder and, in the seventeenth century, their competence expanded to include, among other things, the suppression of witchcraft, prostitution, and gaming. In the course of the seventeenth century the *guba* system became progressively subordinated to the *voevoda* system, and in 1679 the *guba* was abolished. J. L. H. Keep, "Banditry and the Law in Muscovy," *The Slavonic and East European Review*, 35 (1956): 210. Also see H. W. Dewey, trans. and ed., *Muscovite Judicial Texts, 1488–1556* (Ann Arbor, Mich., 1966), 31–43.

[54] For a more detailed description of the process of initiating legal action in Muscovite Russia, see Horace W. Dewey and Ann M. Kleimola, trans. and eds., *Russian Private Law in the XIV–XVII Centuries* (Ann Arbor, Mich., 1973), 3–6.

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Moscow and await further instructions. In cases of alleged witchcraft, the tsar invariably instructed the *voevoda* not only to proceed with the trial but to do so with all haste and vigor.

By the early seventeenth century the post of *voevoda*, or military governor, had become a powerful and universal institution in Muscovite Russia. The Military Chancellery (*Razriad*) appointed the *voevoda*, and he sent petitions to Moscow and received his instructions from the tsar through the *Razriad*. In addition to full administrative powers, the *voevoda* administered justice. The *voevody* in a few of the larger towns had full jurisdiction over criminal matters, including witchcraft. In most instances, however, they could judge criminal cases only on *doklad*.[55] Assisting the *voevoda* was the undersecretary, or official recorder (*pod'iachii*), who handled all of the paperwork in the courtroom. The *pod'iachii* drew up an official record of the trial to be forwarded to Moscow on *doklad*. Since there were no lawyers (in the West European sense) to represent the litigants,[56] both the plaintiff and the defendant answered directly to the *voevoda*.[57]

The trial began with the appearance of the defendant in court where he heard the charges against him and could respond to them if he wished. In the witchcraft trial records, a bailiff seldom summoned defendants. Most appeared in court voluntarily, although one case in Nizhnii Novgorod in 1649 required a small detachment of ten *streltsy* with a bailiff to produce the wife and children and to inventory the personal property of an accused warlock who had been released on surety bond, presumably to guarantee his appearance in court. The bailiff's precaution proved fully justified; when he arrived in the village of the accused he faced an angry mob wielding hatchets, spears, and bows and arrows.[58]

Even if the suspect pleaded guilty to the charge of witchcraft, the trial was far from over. Its emphasis now shifted from the defendant to the broader question of a possible conspiracy of witches. On instructions from Moscow, investigators tortured him and asked typically leading questions: From whom did you learn your witchcraft? Where are your mentors now? How long have you been practicing witchcraft? How many others, in addition to the plaintiff, have become victims of your witchcraft? Have any of your former victims died? What kind of objects (roots, potions, herbs, written incantations) did you use in your evil work? From whom did you acquire these?[59] If during interrogation the defendant in any way implicated others in his practice of the black arts, both past and present, all of those named were summoned into court for questioning and torture. The open-ended nature of the Muscovite

[55] For a good description of the evolution of the *voevoda*'s legal responsibilities and functions, see F. Dmitriev, *Istoriia sudebnykh instantsii i grazhdanskago appelliatsionnago sudoproizvodstva ot Sudebnika do uchrezhdeniia o guberniiakh* (Moscow, 1859), 71–89.

[56] A litigant could be represented in court by an agent; see Dewey and Kleimola, *Russian Private Law in the XIV–XVII Centuries*, 41.

[57] Some of the witchcraft cases were heard before two *voevody* who jointly administered the larger provinces.

[58] Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, viii–xxvi.

[59] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 9, 11–12, 14–15, 25–26, 37, 63, 73, 87, etc.

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witchcraft trials prompted Novombergskii to conclude that the authorities initiated these trials not so much to redress the grievances of individual litigants as to unmask some real or imagined coterie of witches in a given region.[60] From a strictly juridical or procedural standpoint, the seventeenth-century trials in Russia differed little from those in Western Europe, where torture was also often used to build up long chains of accomplices for further prosecution.

Defendants—whether male or female—who pleaded innocent or refused to cooperate in court also underwent torture.[61] The most common form was knouting, thirty or forty lashes at a time. If repeated three or four times without satisfactory results, authorities employed progressively harsher methods, including the rack, the strappado, hot-iron branding or burning wedges (that is, inserting pieces of wood between the victim's hands and feet and lighting them), pincers (used to squeeze the scrotum), and water (pouring a small incessant stream of cold water upon the head of the accused)—apparently a particularly unbearable form of torture.[62] Howard Charles Lea maintains that in Muscovy during the sixteenth century water was frequently used to extort confessions from suspected witches, but only one reference to it survives in the seventeenth-century trial records.[63]

As the trial progressed, both the plaintiff and the defendant presented witnesses, who were called upon to establish the reputation of the accused or to verify the testimony of both litigants. Because authorities regarded confession as the simplest and most desirable kind of evidence, they not infrequently extracted false confessions, implicating innocent individuals, under torture. Physical evidence—herbs, potions, and the like (even such commonplace substances as garlic and anise, with the proper spell cast over them), and any written spell or incantation—was the most damaging, especially if found on the person or in the house of the accused or if traceable to the defendant.[64] Less persuasive and less common in the Muscovite trials were spectral evidence and the classic formula of "anger followed by mischief," although both figured very prominently in witchcraft trials in the West, particularly in Salem Village in 1692.[65] In 1654 a woman was seized, tried, and imprisoned in Moscow on the strength of spectral evidence alone. Her accuser

[60] *Ibid.*, xxix.

[61] For a vivid description of seventeenth-century Muscovite torture methods, see Kotoshikhin, *O Rossii, v tsarstvovanie Alekseia Mikhailovicha*, 94–95. Also see Samuel H. Baron, trans. and ed., *The Travels of Olearius in Seventeenth-Century Russia* (Stanford, Calif., 1967), 220–332. Pregnant women could not be tortured without a special *ukaz* from the tsar; see Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 191.

[62] According to Paulus Jovius, "In the investigation of witchcraft they pour cold water on the head of the suspect; they say that this form of torture is unbearable." See "Posol'stvo ot Vasiliia Ioannovicha, Velikago Kniazia Moskovskago, k Pape Klimentu VII-mu," *Biblioteka inostrannykh pisatelei o Rossii*, 1 (St. Petersburg, 1836): 48.

[63] Lea, *Superstition and Force* (Philadelphia, 1870), 397; and Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 221.

[64] Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 191; and Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, xci.

[65] As defined by Boyer and Nissenbaum in *Salem Possessed: The Social Origins of Witchcraft* (Cambridge, Mass., 1974), 16, spectral evidence was "testimony about supernatural visitations from some demonic creature—perhaps Satan himself—who appeared in the specter (that is, shape) of an accused witch."

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claimed that he had seen the woman in a vision and that she threatened to do "unheard of things." Interpreting this to mean malefic witchcraft, he reported the incident to the authorities. The woman subsequently petitioned the tsar to order her release since the evidence against her was both empirically unverifiable and less than credible, coming as it did from a confirmed drunkard. The outcome of the case is not known.[66]

In at least three cases, however, the evidence presented did revolve around the formula "anger followed by mischief." In 1628, two stablemen from a village outside of Moscow petitioned the tsar, claiming that their respective wives had been bewitched and made insane by a local witch. The insanity closely followed threats made by the accused woman. In his instructions to the presiding judge, the tsar asked that the court make every effort to learn the exact words used to threaten the two victims. In a celebrated case from 1648 (which will be examined more closely later[67]) the following threat was entered as evidence against the accused: "I will make him as black as the ceiling in the *izba* [peasant's hut]; he will become as stooped, as the sickle is bent." And a dragoon was charged with witchcraft in 1677 after he had threatened the wife of another dragoon and she subsequently became ill.[68]

With all the testimony completed, the court undersecretary compiled a written record of the trial for submission to Moscow on *doklad*, a process which could take several months or longer. In the interim the defendant was either released on bond, if he could find sureties, or he was imprisoned.[69] In most criminal cases, including witchcraft, this referral—to the tsar or boyar *duma* for decision—was a routine part of the legal procedure; the *doklad* did not in any sense function as appellate procedure. According to one scholar, referral of the case to Moscow was necessary also because certain crimes, such as witchcraft, were not covered by existing Muscovite laws, only by general decrees. As a result, local judges had to turn to the lawgivers themselves for help in determining the punishment in such cases.[70] Not until the eighteenth century did witchcraft (*koldovstvo, charodeistvo*) become an object of secular legislation in Russia. In his Military Statute of 1716, Peter I forbade witchcraft and related occult practices (*chernoknizhestvo*) under penalty of death.[71] In 1731, during the reign of Empress Anna, the Senate issued an order that condemned witchcraft as a fraud, although its practice was still punishable by

[66] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 83–85.

[67] See page 1203, below.

[68] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 12–14, 56, 110–11.

[69] Suretyship was an important institution in seventeenth-century Russia. For background information, see Marc Szeftel, "The History of Suretyship in Old Russian Law," *Sûretés personnelles: Recueils de la Société Jean Bodin*, 29 (1971): 841–66; and Horace W. Dewey and Ann M. Kleimola, "Suretyship and Collective Responsibility in Pre-Petrine Russia," *Jahrbücher für Geschichte Osteuropas*, 18 (1970), 337–54. For some examples in translation of seventeenth-century surety bonds, see Dewey and Kleimola, *Russian Private Law XIV–XVII Centuries*, 247–54.

[70] Dmitriev, *Istoriia sudebnykh instantsii*, 142. There are no references to witchcraft in either of the *Sudebniks* (1497 and 1550) nor in the *Ulozhenie* of 1649, probably because witchcraft was covered by existing Church legislation.

[71] *PSZ*, 5: 320–21 (chap. 1, article 1). According to Vladimirskii-Budanov, *Obzor istorii russkago prava*, 367, Peter's legislation reflects concern over an increase in witchcraft in the first quarter of the eighteenth century.

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burning at the stake. The same order subjected those who patronized witches and warlocks to severe knouting. But during the reign of Catherine II a more enlightened official attitude toward witchcraft became manifest. Less severe punishment befitting the crime of fraud replaced the death penalty, and the courts received instructions to try cases of alleged witchcraft as they would handle fraud rather than fall under the influence of evidence which could be labeled as popular superstition.[72]

In 1775 Catherine promulgated governmental reforms that called for, among other things, the creation of so-called *sovestnye sudy* (literally courts of conscience). These courts had jurisdiction over cases involving popular superstition (such as witchcraft), juvenile offenders, and the criminally insane.[73] The most frequent cases regarding popular superstition brought before the *sovestnye sudy* involved *klikushi*—women who claimed to be possessed—who ranted and raved hysterically during church services, calling out the names of those who they said had bewitched them.[74] Sometimes the accusations of the *klikushi* resulted in the interrogation and torture of many innocent people. In 1670, for example, some seventy *klikushi* virtually terrorized the town of Shuia with their indiscriminate accusations, which ultimately forced local authorities to petition the tsar for help.[75]

TRIALS FOR WITCHCRAFT WERE INITIATED, as indicated earlier, both by authorities invoking the inquisitorial method and by individual plaintiffs using the accusatory procedure. In the inquisitorial trials the authorities concentrated primarily on an individual's potential to inflict *porcha*—mysterious injury[76]—on someone. The charge in such cases frequently stemmed from simple possession of some herb, root, grass, potion, or written incantation. Seven such cases were tried.[77] Anyone with a reputation for occult knowledge likewise became a prime suspect in the eyes of the officials, who believed such a person capable of inflicting evil at will on unsuspecting individuals. One lengthy case involved a Tatar woman seized and tried in the province of Lebedianskoe in 1630. Her reputation for fortunetelling and healing was so widespread that a crowd always followed her wherever she went.[78]

[72] *PSZ*, 8: 465–66; 19: 23–25; and 21: 480, 485.

[73] Certain other cases also came under the jurisdiction of the *sovestnye sudy*. In addition they acted as watchdogs over the rights of those arrested and held without formal charges: a person held for three days without being charged could appeal to a *sovestnyi sud*. A. Kizevetter, "Sovestnye sudy pri Ekaterine II," pt. 2, *Golos minuvshago*, 2 (1923): 14–28.

[74] *Ibid.*, pt. 1, 1 (1923): 142–55.

[75] Levenstim, "Sueverie i ugolovnoe pravo," pt. 1, 329; and V. A. Borisov, *Opisanie goroda Shui i ego okrestnostei* (Moscow, 1851), 339–40.

[76] From the verb *portit'*, meaning to destroy, deface, corrupt, pervert. Max Vasmer, *Russisches Etymologisches Wörterbuch*, 2 (Heidelberg, 1958): 411. For an interesting and detailed discussion of *porcha* in its magico-medical context, see G. I. Popov, *Russkaia narodno-bytovaia meditsina* (St. Petersburg, 1903), 25–35. Also see page 1188, above.

[77] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 9–12, 33, 73–75; Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, xxvi–xxviii; and Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 157–61.

[78] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 25–33.

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More numerous were the cases brought to trial at the behest of individual plaintiffs seeking redress for serious evil inflicted upon them or those close to them. The *maleficia* or *porcha* in these accusatory procedures fall into six basic categories: taking life, maiming, causing illness, depriving of reason, inflicting impotence, and causing crop failure. Nine—the largest number—of these cases involved alleged life-taking by means of witchcraft.[79] One classic case of death by witchcraft occurred in the town of Dobroe in 1676.[80] A priest brought his two servants, a husband and wife, before the town officials and charged that they had bewitched his wife and daughters to death. As evidence he presented some roots and potions that he claimed he had found in the servants' quarters. The accused woman admitted mixing the potions into the food and drink of the priest and his family. Her husband confessed that his stepfather had concealed some roots and earth in a corner of one of the priest's rooms to make the priest and his family "wither and die." Both the stepfather and his wife were brought in for questioning and torture. Also brought in, as additional evidence, were six small bags of grass (each bag knotted twelve times), a tablet containing an incantation against gun wounds, and another small bunch of grass. The alleged witches supposedly used the grasses to concoct the potions.

The trial further revealed that the accused servant had "taken out" the footprints—that is, removed the earth from the footprints—of both the priest and his wife and had given it to the stepfather. This earth investigators found hidden along with the grass in the priest's house. The servant's wife admitted stealing and having her mother-in-law cast a spell over the headdress of the priest's wife. They then placed the headdress under a wooden log with the following incantation: "Just as this log is heavy, so may this headdress weigh heavily on the head of its bearer." The accused also cast various other spells either over scraps of the victims' clothing or upon their person. The servant, his stepfather, his wife, and his mother were all imprisoned while the case was sent up on *doklad*. Unfortunately, the trial records stop here and provide no verdict or sentence. Considering the incriminating confessions and other accumulated evidence, it seems likely that the accused were found guilty of witchcraft and were sentenced to be burned at the stake.

Not all of the cases involving the taking of life by means of witchcraft were so clear cut. In some the line between malefic witchcraft and folk medicine gone awry is blurred.[81] The people generally believed that certain mysterious forces in nature could be used to cure disease, could be harnessed only by a few wise individuals to whom the people could turn in times of medical need. If the cure worked, the witch gained faint praise; if it failed, the witch faced indictment, torture, exile, or even death at the stake.

One of the more sensational trials took place in the district of Komaritskaia

[79] *Ibid.*, 3–9, 14–25, 53–63, 94–106; Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, viii–xxvi, xxxviii–xxxix, lxxxvi–lxxxvii; and Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 150–51.

[80] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 99–106.

[81] *Ibid.*, 14–25; and Novombergskii, *Vrachebnoe stroenie v do-Petrovskoi Rusi*, xxviii–xxxiv.

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in 1648. A dragoon named Fed'ka Filipov brought suit against Dar'ia Nekrasova, the wife of a village cantor, alleging that nine years earlier she had made him impotent.[82] Two *voevody* held a preliminary hearing on the evidence, which revealed that Dar'ia's reputation as a witch was so widespread that young men from miles around assiduously avoided taking their marriage vows in the village church for fear of becoming impotent. The court ordered the release of both the plaintiff and the defendant on surety bond pending further instructions from Moscow. Fed'ka posted bond, pledging that he would appear in court when summoned.[83] Dar'ia, unable to find sureties, remained in prison. Within two weeks the tsar issued an *ukaz* ordering a full investigation and vigorous prosecution of the case. The court immediately dispatched an investigator to hold inquests (*obyski*) regarding the accused in the surrounding communities.[84]

One of the first witnesses was the parish priest from the village of Morevo, the home of both litigants. He testified that in 1639 Dar'ia had threatened a man who subsequently became emaciated and died. He then recounted the circumstances under which Fed'ka had ostensibly been made impotent. The plaintiff and his new bride had returned from church and retired to an upper wedding chamber to consummate their union. The accused, so the priest had heard, took a splinter of wood, lit it, placed it under the staircase used by the newlyweds, and urinated upon it. A total of 142 individuals came forth to testify against Dar'ia. Some recalled instances of anger followed by misfortune which they had either experienced or witnessed. Others related stories, mostly hearsay, attesting to the evil powers of the woman. No one testified on her behalf, though some maintained that they did not know whether or not Dar'ia was a witch. Her fate, too, is unknown, since the trial record ends abruptly here.

By contrast, the verdict and sentence provide the focal points of the documents for a famous trial in 1647 and 1648.[85] In August of 1647 an oral complaint was filed with the administrator of the estate of Prince Nikita Ivanovich Odoevskii in which one of the estate peasants charged the woman Agaf'itsa Savkina with witchcraft. The accused had supposedly made many people ill by giving them tumors and had caused the death of several others. Her sister Ovdot'ia and Ovdot'ia's father-in-law, Tereshka Ivlev, had assisted in the alleged *porcha*. The case was initially tried at the local level before the estate manager, a *starosta*, and representatives of the community.[86] Witnesses came forth to testify that the accused had made people ill by reciting incantations over them—examples of which are quoted verbatim in the trial record—and by putting grave dirt in their drink. Local authorities referred

[82] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 53–63.

[83] The full text of Fed'ka's surety bond is incorporated into the trial record. *Ibid.*, 54–55.

[84] When witnesses were unable, for some reason, to appear in court investigators could be sent out to take their testimony. Kleimola, "Justice in Medieval Russia," 43.

[85] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 63–73.

[86] The *Sudebniks* of 1497 and 1550 stipulated that local authorities or agents should not render justice without the presence in court of an elected *starosta* and other representatives of the local population.

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the case to the *voevoda* of Shatskoe, who heard additional evidence. Meanwhile one of the accused, Ovdot'ia, fled never to be heard from again. Finally, the case was sent to Moscow where, on November 17, 1648, the boyar *duma* declared the accused guilty of witchcraft and sentenced them to be burned at the stake. Prior to the execution, they were to see a priest and receive Holy Communion. During the disposition of the case in Moscow, a new *voevoda* took office in Shatskoe. When the verdict and sentence arrived, the new *voevoda* was apparently reluctant to proceed with the execution without a specific directive from the tsar. The sources do not say whether or not his reluctance resulted in a reprimand or punishment. In the West, magistrates unwilling to deal with witchcraft cases often found themselves accused of witchcraft.

Because of certain unique elements, two other cases merit special attention. The first, in 1663, involved the trial of five male witches (one of whom was the grandson of an abbot) in Lukh.[87] The town fathers brought suit against the five because they had allegedly bewitched many local women and children, some of whom had died. In addition to the confessions extorted by torture, the evidence against the accused also included several written incantations. The most curious of these reads, "I renounce our creator Christ God, the Church of God, the Most Holy Liturgy, Vespers, Matins, and all godliness, my father and mother, my family and ancestors, and I swear allegiance to Satan and his beloved servants."[88] This incantation, which has the ring of an oath, is the only surviving evidence of Satanism or demonology introduced in the Muscovite witchcraft trials.[89] The sources say nothing about the incantation except that one of the accused wrote it. With another of the accused on whose person written incantations were found, the author was ordered imprisoned under heavy guard pending additional investigations and inquests. The three other defendants were acquitted and released after the initial phase of the trial.

The second case, in 1667, involved the burning of six witches in Gadiach.[90] This multiple witch burning was ordered by Ivan Martynovich Briukhovetskoi, the military commander (*hetman*) of the Zaporozhie Cossacks and civil governor in Gadiach. Unfortunately, no record of the trial has survived, only eyewitness accounts. The six women, one the wife of a colonel, had allegedly caused illnesses of the *hetman* and his wife. Rumor also had it that the accused had "snatched" a child from the womb of the *hetman*'s wife and had cut off one of her ears. In addition, a report circulated that, during the women's imprisonment, cats and mice issued forth from their cell and disappeared into the town. These might well be identified as "familiars," so common in the

[87] Novombergskii, *Materialy po istorii meditsiny v Rossii*, 4: 197–220.

[88] *Ibid.*, 208.

[89] The absence of Satanism in these trials is the more puzzling since the demonic theme was already quite popular and well developed in Muscovite literature by the late sixteenth century. Stories about Savva Grudtsyn and Salomonia the Possessed, for example, reflect this demonic theme. For texts of both stories, see N. Kostomarov, ed., *Pamiatniki starinnoi russkoi literatury* (St. Petersburg, 1860), 153–92. For an English translation of "Savva Grudtsyn," see Serge A. Zenkovsky, ed. and trans., *Medieval Russia's Epics, Chronicles, and Tales* (New York, 1974), 452–74.

[90] Novombergskii, *Koldovstvo v Moskovskoi Rusi XVII veka*, 94.

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witchcraft tradition of England as well as other countries. Both of these cases are intriguing because they bear such striking resemblance to the witchcraft tradition of the West. Satanism, familiars, infant body snatching, multiple burnings—the records of European witch trials are replete with these. They may reflect Western influence seeping into Muscovite Russia from Poland in the second half of the seventeenth century. Be that as it may, these darker and more bizarre manifestations of witchcraft did not find wide currency in Russia.

WHERE DO THE RUSSIAN WITCHCRAFT TRIALS of the seventeenth century stand in relationship to the so-called witch craze of Western Europe? To begin with, these trials did not occur without preparation: witches had long been sporadically persecuted in Russia. This persecution coupled with the witch paranoia that periodically gripped the Muscovite court apparently laid the groundwork for the trials of the seventeenth century. From the tsar down, the authorities and the people firmly believed that certain individuals had the ability to inflict *maleficium*—or *porcha*—on unsuspecting victims. This belief received elaboration and juridical sanction in the local and provincial courts, which used torture and encouraged false accusations (ultimately resulting in uncovering a chain of accomplices). Some scholars, like Novombergskii, have tried to compare the Russian trials to those of the West in their cruel and inhuman treatment of suspected witches. Only the scale on which the Western trials were conducted, they maintain, sets them apart from their Muscovite counterparts. But this judgment is much too harsh. Not only were a comparatively small number of victims tried and burned, but the witch persecutions in seventeenth-century Russia demonstrated a sense of measure and restraint largely absent in the West. Thus, the quantitative difference of which Novombergskii has spoken can be translated into a qualitative one as well. What carries the label "witch craze" in the West might more accurately be termed only a "witch scare" in Muscovite Russia.

A number of scholars have eloquently addressed themselves to the question of what made the witch craze possible in the West.[91] To date no one has been able to solve the problem of causation convincingly. Following the anthropological school, Alan Macfarlane has persuasively argued that periods of social stress have frequently been marked by an increase in suspicions concerning witches and witchcraft.[92] This interpretation has particular relevance when applied to medieval Russia where, as we have seen, there was a close relationship between natural calamity (drought, famine, plague) and the incidence of persecution for witchcraft. This persecution manifested itself most vividly in

[91] Among the most recent such studies, those of Trevor-Roper, Cohn, Thomas, and Boyer and Nissenbaum have already been cited. One must add to this list the works of Alan Macfarlane (*Witchcraft in Tudor and Stuart England* [New York, 1970]), H. C. Eric Midelfort (*Witch Hunting in Southwestern Germany, 1562–1684* [Stanford, Calif., 1972]), and E. William Monter (*Witchcraft in France and Switzerland: The Borderlands During the Reformation* [Ithaca, N. Y., 1976]).

[92] Macfarlane, *Witchcraft in Tudor and Stuart England*, 249.

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the swimming ordeal, used to ferret out witches in drought-stricken areas as early as the mid-twelfth century. Potentially, then, Russia was well on the way to developing the techniques and evolving a tradition of witch hunting quite early in its history.

Hugh Trevor-Roper has suggested that pressure for persecution in the West did not come from the Church or the government but from those low on the social scale who needed a "scapegoat for social frustration."[93] If this is true, seventeenth-century Russia was surely ripe for a witch craze. By the late sixteenth century, the *oprichnina* of Ivan IV had brought about a massive dislocation of the population. Then came the Time of Troubles with its foreign invasions, numerous pretenders, a devastating famine (1601–03), and a mass popular uprising (1606). These troubles were in turn followed by the growth of a "new autocracy" under the Romanovs, the final "fastening" of the peasants to the land and townspeople to their occupations, another round of popular—urban—unrest (1648), and, finally, a great schism within the Orthodox Church (1666). Judged by the extent of its "seventeenth-century crisis" and by its earlier tradition of witch persecution, Russia ought to have experienced a massive witch craze of West European proportions.

Tracing such cause-effect relationships, however, is always a highly speculative endeavor, and Trevor-Roper's view has not escaped criticism. Still, the argument can be advanced that the Russian infection was milder than the West European because of certain peculiarities in Russian religious attitudes. As George P. Fedotov has so ably demonstrated, the core of East Slavic paganism was not the belief in a pantheon of great gods but rather the veneration of nature and of its elements and phenomena.[94] Not only was there widespread acceptance of the notion that certain individuals possessed the power to control the forces of nature—in the words of Bishop Serapion, "permitting rain, bringing warmth, making the land fruitful"[95]—but there was a concomitant readiness to call upon nature (by means of the swimming ordeal, for example) to bear solemn witness when these forces had seemingly been disturbed or tampered with. Among the Eastern Slavs, consequently, the intellectual rationale for witchcraft was predicated on a pantheistic concept of the universe rather than a demonological one, as was true in the West. Without Satan and without the Sabbat, Russian witches bore little—and sometimes no—resemblance to their Continental counterparts. The Russian witch could be tried and punished, much like the English witch, for the secular crime of malign sorcery—but not for heresy.

Perhaps even more significant to the general history of Russian witchcraft was the Russian religious duality: in Russia, much more than in other European countries, paganism and Christianity had coexisted for hundreds of years and had given rise to the phenomenon of *dvoeverie*—or ditheism. This

[93] Trevor-Roper, "The European Witch-Craze of the Sixteenth and Seventeenth Centuries," 114.

[94] Fedotov, *The Russian Religious Mind: Kievan Christianity, the Tenth to the Thirteenth Centuries* (New York, 1960), chaps. 1, 12.

[95] Petukhov, *Serapion Vladimirskii*, 11

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unique feature of Russian spirituality often resulted in a certain religious and cultural ambivalence which helped to shape the popular attitude toward witchcraft. Serapion took note of this ambivalence in his sermon when he chided his countrymen for burning witches to whom the people have just brought gifts and whom they have in the past frequently implored for help. The Chronicle of Novgorod alluded to it in an entry for 1224. After describing the burning of four witches, it added somewhat sceptically, "they [the executioners] thought that they [the victims] were practicing sorcery, but God knows. . . ."[96] And the trial records repeatedly show plaintiffs filing suit against the very witch doctors to whom the accusers had earlier come for a cure.

The irony in all this is inescapable. For centuries the Muscovite Church tried unsuccessfully to persuade the people to abandon their pagan ways, their *dvoeverie*. But, in the seventeenth century, this tradition of dual belief may have counteracted the fear of witchcraft among the people and the readiness of officials to prosecute, helping Russia escape all of the horror, the excess, and the ignominy of a full-scale witch craze.

[96] Michell and Forbes, *The Chronicle of Novgorod, 1016–1471*, 69.

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Reviews of Books

GENERAL

MELVIN J. LASKY. *Utopia and Revolution: On the Origins of a Metaphor, or Some Illustrations of the Problem of Political Temperament and Intellectual Climate and How Ideas, Ideals, and Ideologies Have Been Historically Related.* Chicago: University of Chicago Press. 1976. Pp. xiii, 726. $35.00.

This prodigiously overwrought essay on the conjuncture of the utopian impulse and the modern religion of revolution is not so much a history as a sermon from a disenchanted radical. Melvin J. Lasky revives the warnings of Orwell, Camus, and all their nineteenth-century precursors "against the intoxication of abstract political ideas and the self-deception about an absolute social triumph" (p. 429). His message is that extremism always fails. Only when the utopographer offers "a supple changing ideal of diverse virtues and dreams" and the revolutionist trades in his millennial delusions for a program of tolerant and humane reform, does "history provide consolation" (p. xii).

Lasky seems to believe, if one may paraphrase the Republican candidate of 1964, that extremism in the defense of moderation is no vice. His book overwhelms its readers with passionate, undisciplined, often incendiary prose, in which the autobiographical element always lurks just below the surface. As he himself points out, *Utopia and Revolution* "is not an academic exercise" (p. 51). What remains of the Trotskyist youth of the 1930s in the mellowed publisher of *Der Monat* and *Encounter* is a taste for violent metaphor.

No ordered résumé could possibly convey what the book is all about. But let us try. There are three large parts: a trio of chapters on utopia, revolution, and dogmatism in modern history, which amounts more often that not to a liberal critique of Marxism; a longer middle section devoted mostly to the utopian visions of the age of the English Civil War; and a third part entitled "Ideologies," which dwells on the revolutionary political programs of the English Civil War and the English response to the French Revolution, with a final chapter on the glorious ambiguities of Immanuel Kant. Throughout, the author trains his eye on the point at which utopian vision and revolutionary commitment intersect. He makes no effort to provide a synoptic history of utopia or of revolutionary movements as such. Only their interaction commands his interest, which is perhaps the uniqueness and value of the whole project.

But it remains a work more of argument than of scholarship. Lasky's pages brim with heroes of critical moderation (e.g., Bayle, Bernstein, Camus) and "paradise-seeking" villains of messianic dogmatism (e.g., Joachim, Comenius, Lenin). He quotes at length from all his writers, catching them at their brightest moments, with a literally dizzying effect. Ideas, and above all the nuances and contradictions of ideas, fascinate him, but he lacks the intellectual historian's power of synthesis. The nurturing political and socioeconomic context is missing. A sense of development or even chronology is missing, as Lasky dances delightedly back and forth across the centuries. In a typical passage, he shifts from Sarpi and Campanella, to Erasmus and Calvin, to Spinoza, to Lukács and Bloch, to Turgot, to More, to Djilas and the Hungary of 1956, back to More again, and finally to Voltaire and Malesherbes, all in 14 pages.

Often he scores with a delicious irony, as when the villain Kautsky berates Bernstein for being "too English" and later in the same paragraph the villain Crossman denounces Crosland for being "too German," both epithets meaning the same bad thing in different eras of dogmatism. Or there is the pathos of Marx's predecessor Wilhelm Weitling reduced in his old age to tailoring for the Fisks and the Vanderbilts. Such ironies abound in Lasky's essay. Men and their ideas are always going "full circle," and agonizing paradoxes are as common as dandelions in spring. But then, after all, as Lasky explains in his preface, his goal in writing *Utopia and Revolution* was only to "offer some intellectual pleasure and general suggestiveness" (pp. xi–xii). That much he has surely done.

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MAURICE MANDELBAUM. *The Anatomy of Historical Knowledge*. Baltimore: Johns Hopkins University Press. 1977. Pp. viii, 230. $12.95.

On the evidence of this book, the impasse between practicing historians and historicizing philosophers continues. Anxious to go forth in pursuit of the multitudinous facts, convinced that he needs only his native insight and intelligence to organize this data, and perhaps eager to test some emotionally live hypothesis, the historian receives with impatience what strikes him as tedious and superfluous discussion of theory of history, which only occasionally sheds light on his real problems.

The distinguished philosopher of history, Maurice Mandelbaum, does not even say his book is for the edification of historians, so we have no right to assume it is. Its context is a continuing debate within Anglo-American philosophical circles, which has been going on since about 1942. (One wonders if the philosophers, through being poor historians, do not overestimate the modernity of these issues, which were canvassed with considerably more urbanity in the nineteenth century by Bradley, Froude, Dilthey, Rickert, Fustel de Coulanges, and others.) It is true that the philosophers took up matters suggested by historians, who fell onto problems of causation, objectivity, generalization, and necessity, which they then mangled in occasional discussions. (The objectivity debate began with Beard.) But the philosophers have gone their own way, dominated by methods and values peculiar to philosophers, gradually losing the "working historian" who cannot stay for that relentless pursuit of an argument which is the trademark of the philosophical mind.

In general, Mandelbaum's book is neither systematic nor attractively written enough to serve as a basic textbook of philosophy of history for the historian. It does review the leading issues, especially causation and objectivity, with much awareness of the specialist literature, but also with a marked concern to define the author's particular position in controversies with other philosophers, to an extent that loses the main issues in academic pettifoggery. (Historians are, of course, guilty of this too in their in-house arguments.) The most notable thesis, an allegedly significant distinction between "general" and "special" histories, will not seem very convincing or meaningful to historians, I fear. In Mandelbaum's classification, a general history is one that deals with the whole of a society at a particular place and time, whereas a special one handles a particular branch of human activity or knowledge such as literature, art, science, technology. But much of what historians write is neither general nor special history in Mandelbaum's terms, obviously. Moreover, it is by no means clear that there is so much difference between the two as he alleges; for example, he argues that general histories are more objective because forced to take account of the existing structure of a society whereas special histories need not (pp. 160–66). In practice, surely, there is opportunity for even more arbitrary selection and analysis in a "general" than in a "special" history.

Mandelbaum is consistent with his position in earlier books in arguing for a greater degree of "objectivity" in historical knowledge than many would accept. Historians will probably approve, but may regard as trite, his conclusion that agent-causation and necessary conditions are intermixed in historical explanations. They may wonder why he denies (p. 92) the status of negative causes: why is what men failed to do—like the Dual Monarchy not appeasing Slav demands for autonomy—not an important explanatory factor? It is abstract reasoning and not attention to what historians find that seems often to dictate such conclusions. Again, Mandelbaum's insistence that "sequential" (narrative) history is separate as well as different from explanation, and both from interpretation (pp. 26 ff.), sounds false and labored. Surely these, too, are enmeshed together in most historical writing. But his distinction between objectivity in method and in knowledge (pp. 146–51) is illuminating.

All scholars with an interest in such questions will want to read and ponder these closely reasoned arguments by one who has long meditated on them. Whether most historians will do so is another question. They should, but a presentation more attuned to their mentality than this one would serve better. We still need more encounters at the interface of history and philosophy of history, rather than continuing to launch attacks from deep within each fortress.

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LOUIS J. HALLE. *Out of Chaos*. Boston: Houghton Mifflin. 1977. Pp. xiv, 657. $20.00.

Through metahistory, speculative thinkers have long sought to find an overarching pattern in the past and to discern the future. In our time the two most illustrious practitioners of this craft have been Spengler and Toynbee. Now Louis J. Halle has joined the field, and although he reaches the same conclusions as Spengler and Toynbee, he does so in a very different way. Rather than starting with the beginning of history, as they did,

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Halle goes clear back to the origins of the cosmos. Only after an extended consideration of the physical universe and of biological evolution does Halle arrive at history. Once there he discovers, as Spengler and Toynbee did, that civilizations run in cycles.

If this is not original, Halle's treatment of the physical and biological context of history is. He vividly conveys a sense of the physical insignificance of our planet and of the constantly accelerating character of biological evolution. His argument that the accelerating character of biological evolution has led in turn to the accelerating character of history has force. Halle also offers tantalizing analogies between science and history. Heisenberg's uncertainty principle, he suggests, works for small-scale historical events as well as small-scale physical ones, and Bohr's principle of complementarity can reconcile conflicting views of history as well as of natural phenomena.

While Halle finds a majestic order in nature, he sees a restless dynamic to history. He portrays history as a struggle between man, who builds order, and pernicious forces of change, which tear down what man has built. Halle scarcely bothers to discuss these forces of change, but he does an excellent job of showing how man creates order. His examples are wide-ranging—they come from the spheres of religion, philosophy, art, and science—and are presented with care and sensitivity.

The book does have serious deficiencies. Halle's bibliographical base is slender and his discussion of the history of past civilizations is often cursory to the point of caricature. There are also occasional outright errors. Halle speaks, for instance, of planets as moving in circular orbits (p. 36) and makes the startling claim that Aristotle made a "superficial contribution to medieval philosophy" (p. 468). Elsewhere serious matters are dealt with lightly. Marxism is brushed aside as a new religion at one point, and at another, Halle asserts that it is beyond the power of a historian to do more than guess at what historical agents think. In neither case is the argument pursued any further.

These are several of the flaws in a book with a noble purpose: to see man and his history in the broadest possible perspective. Halle has not succeeded in his attempt to produce an overview of knowledge, but he admits as much in a passage in which he compares himself to Faust forlornly brooding in his study. Even if Mephistopheles did not come to lend a hand, Halle has given us a book with literary merit, and one that makes a legitimate demand of us: that we occasionally emerge from the confines of our discipline to view history in the greater context of human knowledge.

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ARNOLD PACEY. *The Maze of Ingenuity: Ideas and Idealism in the Development of Technology*. New York: Holmes and Meier. 1975. Pp. 350. $12.95.

Derived from Arnold Pacey's lectures at the University of Manchester Institute of Science and Technology, this book briefly surveys the history of technology from 1100 to about 1870. Its message—not particularly new to historians of technology but nevertheless stimulating to all historians—is that not all technology derives from materialist or utilitarian objectives. Instead, Pacey strives mightily to demonstrate the importance of "idealism" as a "driving force for technical change—idealism embracing social or religious purposes, or even abstract symbols and intellectual schemes" (p. 41).

Unfortunately, Pacey is not always able to prove that humanitarian and social ideals have been more of a dynamic force for innovation than have, say, economic or military stimuli. Although he can fit medieval cathedral-building into his idealistic theme (and without even referring to Henry Adams!), he finds it more difficult to trace the influence of noneconomic impulses on the Industrial Revolution, when, he admits, the two major technical incentives were to cut costs and to search for more power (p. 205). Yet he still finds evidence for his "idealistic" thesis in the "philanthropic technology" of Robert Owen and William Strutt (fireproof factories, better heating and ventilating for hospitals).

Pacey's other major theme is the importance of ideas in technological development. During the Scientific Revolution, for example, he finds a linkage between technology and ideas in the "long-standing relationships between mathematics and 'public works' technology associated with architecture" (pp. 129–31), and in the development of "mechanical philosophy" (p. 173). During the Industrial Revolution, he finds the development of a "rational approach to engineering" as demonstrating the ties between ideas and technical methods. This rational approach did not derive from any basic knowledge of scientific principles, but because familiarity with machines led people to become "mechanically-minded" and eventually to formulate mechanical problems in generalized and abstract terms. (Apparently, specific solutions and concrete formulations do not constitute "ideas" in Pacey's lexicon.)

Whether or not one agrees with Pacey's theses, his emphasis on ideals and ideas provides interesting insights into some elements of technological development which are often given short shrift in standard accounts. The book would make good collateral reading in a survey course in the history

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of technology when used alongside fuller and less dogmatic texts.

MELVIN KRANZBERG
Georgia Institute of Technology

JOHN D. POST. *The Last Great Subsistence Crisis in the Western World*. Baltimore: Johns Hopkins University Press, 1977. Pp. xv, 240. $12.95.

This learned and persuasive monograph seeks to assess the consequences for Europe and the United States of widespread crop failure in the years 1816–17. After establishing volcanic activity and resulting dust clouds as the likely cause of unseasonably cold and wet weather, John D. Post describes the consequent dearth of cereals in all parts of Latin and Germanic Eruope, surveys popular responses—bread riots, vagrancy and emigration—and then takes up the demographic and trade cycle repercussions that followed from the food shortages. A final chapter attempts to connect political policymaking in France and Germany with the social and economic instabilities created by two successive bad harvests.

Such a bald summary does not do justice to the dense mass of information the author has crammed into this little book. Statistical tables, some of them constructed from many divergent sources, support a text that is often so closely packed as to sacrifice easy readability.

In addition to information, the book has a thesis, to wit: that "progressive rationalization of political administration and improved economic productivity . . . together brought about the abatement of famines and . . . the taming of severe epidemic disease" (p. 123). Hence, by comparison with crises created by crop failures in previous centuries, the enduring disasters of 1816–17 were trivial, although in those parts of Europe where efficient government and economic productivity had not advanced very far, famine and disease manifested some of their old lethality.

Ironically, the considerable success with which European governments and societies were able to cope with grain shortages in the early ninteenth century has had the effect of distracting historians' attention from the whole episode. Fascinated by the emerging industrial society of the age, scholars have failed to give appropriate weight to the price of bread as a factor in precipitating urban disturbances or in affecting ups and downs of the trade cycle. Or so Post argues in what I find to be a thoroughly convincing fashion. After all, the majority of Europeans and Americans were still directly dependent on agriculture; food took more than half of the workingman's budget, and when local supplies failed, the means for bringing grain from afar were slow-acting, dependent on winds and ice in the Baltic, as well as on the availability of ships, and on an administrative system that could and would issue the necessary purchase orders in time.

Altogether, this is an admirable monograph that reminds us of the ecological complexities of human life and of how precarious man's hold on the natural environment remained into the nineteenth century. My only significant complaint is that Slavic Europe is almost entirely left out. Russia figures only as an exporter of grain; the internal effects of the bad weather are passed by. In addition, Spain and the Ottoman Empire scarcely appear; but this conventional diminution of the definition of Europe is normal. The author should rather be praised for recognizing the integration of the U.S.A. into the North European world system so that he avoids the usual bifurcation of the North Atlantic economic community into European and American fractions.

WILLIAM H. MCNEILL
University of Chicago

J. L. CLOUSDLEY-THOMPSON. *Insects and History*. New York: St. Martin's Press, 1976. Pp. 242. $18.95.

For decades, historical appraisals of epidemic disease in social context have split roughly into two camps. Accounts of specific disease, such as Charles Rosenberg's *Cholera Years*, looking at the local social and political scenarios which reflect the impact of disease, can be social history at its best. Other accounts of epidemics as determinants of the course of human history have assumed a global scale. Such accounts, lately back in vogue, have assumed an ecological, almost cosmic stance toward the precarious balance between man's aspirations, his follies, and his ills.

Insects and History is a bug's-eye view of Western history in the latter genre. It is fact-filled, anecdotal, and largely uncritical. Insects and men, parasites and hosts, the author asserts, have long danced an intimate *pas de deux*, by turns deadly (to both) and occasionally commercially useful to the "host." The latter role gets short shrift, however, in a final chapter. J. L. Cloudsley-Thompson's emphasis on the host-parasite-environment relationship, where insects are sometimes the parasites and other times, perhaps more commonly, vectors for smaller parasites (viruses, rickettsiae, bacteria), invites comparison with works in the "global" tradition mentioned earlier. Exponents of this tradition stretch from William McNeill in his recent *Plagues and Peoples*, back through the early-twentieth-century works of C. E. A. Winslow and Hans Zinsser, and beyond to the nineteenth-century writings of pioneers like J. F. C. Hecker.

Unfortunately, this book suffers in any such comparison. It lacks both the thematic—some

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would claim dubious—structure ("microparasite versus macroparasite") of a McNeill and the felicity of a Zinsser. While he usually avoids simplistic cause-and-effect formulations, Cloudsley-Thompson, an entomologist by training and trade, is so close to his subject that he loses sight of the forest for the aphids in the leaves of his trees. His thesis, if there is one, seems to be that man and insect are locked into an endless vicious cycle of war, disease, and poverty. It is a tried and true thesis which he illuminates with a vast array of information. But in organizing that information, the author himself remains locked into the cycle which is best characterized in his own words: "a succession of pestilences."

RUSSELL C. MAULITZ
University of Pennsylvania

A. L. ROWSE. *Homosexuals in History: A Study of Ambivalence in Society, Literature and the Arts*. New York: Macmillan Publishing Company. 1977. Pp. xiii, 346. $12.95.

The tone and scholarly content of this book is revealed in the preface when A. L. Rowse tackles the problem of why male homosexuality has been treated more harshly than lesbianism. While there is disagreement, such scholars as Karlen, Bailey, and Bullough have discussed this problem, and other historians have taken into account such matters as cross-cultural differences in the treatment of male and female sexuality, variations over time, and, in Western culture, specific male fears of homosexuality. Such historical differences, however—presumably what history is about—matter little to Rowse. He explains the differences by asserting that "the male is the aggressive animal." After an analogy about "stupid bulls," Rowse informs us that women are "more secretive," that they "keep quiet about their privacies," and finally, that they are "more sensible about life" (p. xii). After that breathless beginning, the stage is set for a long and tedious series of vignettes, heavily overlaid with irascibility and crankiness, which is presumably intended to compensate for poor scholarship.

There is no attempt in this volume to define homosexuality and, as a result, Rowse confuses serious sodomites like Roger Casement and innumerable historical figures who may, for a brief moment in their lives, have cast an erotic eye on a passing youth. Without regard to contrary evidence, the homoerotic feelings of large numbers of poets, novelists, and artists are proclaimed to be *the* significant factor in their lives and the key to understanding their work. In order to reduce artistic accomplishment to homoeroticism, Rowse uses a compost of debased Freudianism, sleazy psychologizing, and a reductionist view of deviance that equates complex personalities to relationships with the mother or to illegitimacy. Erasmus' illegitimacy, for example, makes him an "outsider" and an outsider is "apt to become an observer of society" and to "watch men's follies and brutalities, their passions and conflicts, their fundamental silliness" (p. 8). To understand Henry III, we have only to look at Catherine de Medici: "He was a 'mother's boy'—a psychological pattern we shall see recurring again and again" (p. 32). Rowse adds to the confusion by continual references to masculine and feminine "types"—Henry III was a feminine type and so was T. E. Lawrence. In the case of Lawrence, this is clear because of a "photograph of him taken in India, wide hips, slim waist, gesture of arm and hand held up to the face" (p. 256). Perhaps he was brushing away a fly—but let us not consider such mundane possibilities. Rowse is determined to include as many well-known figures in his homoerotic web as distortion of the evidence will allow.

We get a sense of how Rowse uses evidence when we compare his conclusions on Michelangelo with the more restrained, fair, and scholarly statements of Vern Bullough (*Sexual Variance in Society and History* [1976]). Where Bullough says, "Still, we have nothing to suggest that Michelangelo's activities went so far as physical intercourse" (p. 419), Rowse declares "it is not at all likely that all his inclinations, with so passionate a temperament, were repressed" (p. 20)—assuming that "so passionate a temperament" must find its *major* outlet in sensuality rather than art.

The only justification one might make for a volume like this is on the grounds of "consciousness-raising"—we need a work on great historical foot fetishists or necrophiliacs, so that current practitioners will know that they, too, have a past. Even then, this book is not likely to be popular with homosexuals because of its snobbish, upper-class bias. Rowse continually condemns mass society and longs for something called "aristocratic taste." While obviously pleased with the decline in the persecution of homosexuals in the twentieth century, he still yearns for the good old days of good old taste, when boys were boys and aristocrats were pederasts.

The book has no conclusion, which, in this case, is understandable. It has no discernible hypotheses, premises . . . rhyme or reason.

ARTHUR N. GILBERT
University of Denver

HERBERT N. SCHNEIDAU. *Sacred Discontent: The Bible and Western Tradition*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xiii, 331. $5.95.

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It is not often that readers are treated to such a well integrated work, combining literature, archeology, and anthropology. Herbert N. Schneidau attempts in this volume an analysis of Western culture through the use and understanding of the Bible, starting with the conviction that "we are a far more Biblical society than we admit." While using the resources of dedicated scholarship in many fields, he does not pretend to be expert in them, except perhaps in the field of literature.

Sacred Discontent falls into five sections. Schneidau first discusses the Bible and Western culture under what he calls "Praise of Alienation." His conclusion will not please everyone: "Thus latent Yahwism works within us, leavening all the lump. We are condemned to freedom, not because God is dead but because he is very much alive, as an agent of disillusionment in a basic sense . . . We have reached out for the apple of self-knowledge, and in doing so have alienated God, nature, and each other" (p. 49).

In the second chapter, "The Mythological Consciousness", the author argues that mythology is highly communicable, and he finds evidence of the "widespread religious phenomenon of syncretism"—with Hebrew society as the base. Here he relies heavily on the analysis of Lévi-Strauss. Chapter three, "The Hebrews against High Cultures," examines the ways in which the ancient Hebrews regarded surrounding civilizations—desert, farming, and pastoral. The fourth chapter looks at Biblical and Western views of history, and in the final chapter the author delves into the Bible and Western literature. It is in this concluding chapter that Schneidau demonstrates how Western literature is deeply dependent on the Bible and must be understood in relation to it.

Sacred Discontent is written in a very readable style, making it possible for the reader who is not expert in literature, archeology, or anthropology to follow its argument. One must, however, pose some critical comments on this work. While the book draws largely from the Old Testament, it makes comparatively little use of the New Testament. For the "Western person," the Bible is a combination of both. Further, it is not clear in the book where and when the "Western Tradition" begins and ends: one is alternately plunged into the Mediterranean world, the Middle East, Western Europe, sometimes into North America. It is very noticeable that the author offers no discussion of the Church, which, whatever view the author may hold about it, has been the central point of reference in the use and hence influence of the Bible in the Western world. To omit analysis of Biblical influence through the spiritual, theological, and artistic creativity of the Church is an astonishing and inexcusable omission. It should also be pointed out that the Bible is primarily a religious book both for Jews and Christians, and that its influence has been far greater in religious life than in general literature. Religion is as much (if not more) part of culture as literature.

JOHN MBITI
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HYGINUS EUGENE CARDINALE. *The Holy See and the International Order*. Toronto: Macmillan of Canada. 1976. Pp. xx, 557. $29.95.

The titular Archbishop of Nepte, Apostolic Nuncio to Belgium, Luxembourg and the EEC, has written an indispensable book for anyone who wishes to become a papal diplomat or aspires to the office of *doyenne*. For the rest of us this fat tome is less useful. Historians should note, however, that Monsignor Montini, now Paul VI, supported history as "one of the best disciplines for training the diplomat." An implementation of this papal idea on a secular level might produce a fatter EIB. Cardinale's book is a tedious sort of encyclopedia, laboriously justifying papal diplomacy at every opportunity and generally in terms of its pacifying function, although it also deals with the other numerous aims of papal diplomacy in getting the best possible privileges for the Church. Naturally, such aims are stated in the grandiose language with which diplomats usually hide their thoughts.

After seventy pages dealing with the Church and politics, papal diplomacy in general, and the history of papal diplomacy, the author gives us seven chapters on: the juristic position of the Church and the Holy See under canon law and international law; the juristic position of Vatican City; legation; international organizations; and consular relations. About two hundred pages of appendixes follow: Paul VI's apostolic letter *Sollicitudo Omnium Ecclesiarum* (1969) on the duties of papal representatives, the text of the Lateran agreements (1929; confirmed in 1947), and a miscellany of items, including a list of visits by heads of state to the Sovereign Pontiff from 1922 to 1975 and the texts produced by several international conventions. There is an "essential bibliography" and an index of thirty-four pages.

This is a book full of the useful technical data one finds in reference manuals. In spite of the devotion of the Pontifical Ecclesiastical Academy to history, the author seems remarkably immune from any infection with the new-style diplomatic history associated with Arno Mayer. Although the author has used the works of scholars like Mattingly, he never comes close to the level of historical explanation in *Renaissance Diplomacy*. In a su-

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perb *lucus a non lucendo*, on pages 26–27, Communist political ethics is derived from modern Hegelian immanentism and presented as similar to Machiavelli's thought, substantially adopted by Maurras.

Fortunately, most of the history in the work is peripheral to its basic achievement of providing an analysis of the status of the Church in the international order. Before venturing further in religious history, however, Cardinale should remember his Gibbon: "The theologian may indulge the pleasing task of describing Religion as she descended from Heaven, arrayed in her native purity. A more melancholy task is imposed on the historian. He must discover the inevitable mixture of error and corruption which she contracted in a long residence upon earth, among a weak and degenerate race of beings."

H. W. PAUL
University of Florida

RICHARD SENNETT. *The Fall of Public Man*. New York: Alfred A. Knopf. 1977. Pp. xii, 373, xvi. $15.00.

Richard Sennett outlines a transformation of Western man, particularly residents of capital cities, from public man to private man during the last two centuries. His book sketches two urban types, the citizen of the *ancien régime*, who separated his structured public behavior from his spontaneous domestic life, and the modern individual who fuses his intimate and official personalities. *The Fall of Public Man* argues that changing perceptions of self in society now produce a cult of personality which inhibits the blunt exchange of diverse political views. Within the framework of two theoretical sections, a historical discussion of eighteenth- and nineteenth-century Paris and London assesses an erosion of public life through an analysis of middle-class behavior in the theater and on the street. The author considers many fascinating, often overlooked aspects of city life that he thinks demand "a kind of study apart from the usual modes of social history" (p. 6). The subjects reach from the etiquette of dress to the style of concert violinists, and from the way people were seated in theaters to the way they display themselves on promenades. Richard Sennett brings his understanding of the interaction between actor and spectator to his evaluation of club life, public speech, and political advocacy. Although the book utilizes historical evidence, it is not a historical study. Its identification of narcissistic modern man rests ultimately on observations of contemporary New York City. Its argument aims at reversing current social science theory of the other-directed orientation of man's personality and his search for community.

Theories of social science structure the intricate argument, yet the book does not flourish on them. They order the historical portion from a well-understood position in the present, but the use made of them runs counter to a view of history as man's attempt to understand himself through his past. The changing temper of any age certainly heightens the awareness that written history may or may not be wrong, but *The Fall of Public Man* does not alleviate this familiar dilemma methodically through a rigorous return to and a fresh analysis of historical sources. Instead the author squares the discrepancies of his previous studies with the general framework of the present book as "their historical and theoretical setting" (p. 44). This kind of history, written to buttress theory, derives data from secondary works and arranges them with little concern for their relationship to time and space.

In lieu of any variants of historical method, evidence comes from "posthole" methods that "try to depict the sweep of historical forces and . . . some of the richness of detail which comes from delving into a specific moment" (p. 42). Although the procedure is introduced as the practice of historians, its result speaks against a relationship. Postholing seldom reaches deeply into past human experience and often ranges wide afield. As a tool for explanation it lacks the terse logic of comparative history which must say something about compared units that could not be said by studying each separately. Postholing overlooks a multitude of influences that presumably account not only for similarities but also for differences between Parisians and Londoners. It ignores demarcations derived from politics and economics, sidesteps variations produced by population changes and technology, and touches primarily those aspects of culture that are more readily subject to the insights of social psychology.

The flaws of this approach give a basic character to the book: it slights historical context and lacks historical continuity. The author's conclusion ties twentieth-century New York City, in the guise of Forest Hills, to a discourse that focuses, despite idiosyncratic detours, on eighteenth- and nineteenth-century Paris and London. There is no trace of a historical perspective which would account for the antecedents that made New York City something other than an extension of a European capital city. The discussion of the French stage of the eighteenth century looks only to the Greek and Roman stage of antiquity, without recognizing the precedents for the notion of the world as theater in medieval mystery plays or in Jacques Callot's stage designs of the early seventeenth century. The book makes many excursions into fleeting aspects of culture, yet in its discussion of the

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theater misses the rise of the vaudeville house and music hall as the nursery of a new urban audience. All the bewildering configurations, attached to the behavior of people in the 1750s, the 1840s, and the 1890s, fail to answer the basic question of why changing codes of etiquette led to a rejection of public life.

Instead of providing an answer, the book merely illustrates this question by stressing what is suspected anyhow, that public and private behavior changed between the three decades. Instead of documentation, it relies on plausibility as proof according to "the expectations of a sophisticated, intelligent general reader. If that reader finds," the invitation reads, "a reasonable analysis of how a malady of modern society has come about, the book has succeeded; if after finishing the book, he thinks of an alternative logic for explaining this distress, so much the better" (p. 43). Satisfied with plain logic, this reader shunned "alternative logic" and rejected an assumption about sick civilization based on the attitude of a few of his fellow men towards public and private life. However, he accepts the assumption, reiterated throughout the book, that the city is a lasting ground for cultural expression and social organization.

History may be reduced to historical social science, but writing it remains an art. In that regard the book, which professes intimacy with European theater, music, and painting, lacks the fundamental and enduring quality of that art, to reduce expressions to essentials. The prose is bad. Burdened with jargon, the sentences stagger under the weight of subordinate clauses laden with abstractions. There are moments when bright speculations propel the argument and fascinating descriptions evoke a scene. The discussions of *flâneurs*, searching for the unexpected drama of the street, and of program notes, explaining the action in the theater, are intriguing forms of the concept of the city as stage. The book sparkles with provocative ideas about city life, but these momentary insights do not produce a fresh approach to social history.

GUNTHER BARTH
University of California, Berkeley

VERNARD FOLEY. *The Social Physics of Adam Smith*. West Lafayette, Ind.: Purdue University Press. 1976. Pp. xvii, 265. $11.95.

In increasing order of specificity and decreasing order of plausibility, Vernard Foley's major contentions are: 1.) that Adam Smith's economic doctrines were more directly dependent on classical Greek sources than has been previously acknowledged; 2.) that the classical passages of greatest importance—from Plato, Polybius, and Lucretius—focused on analyzing social and economic developments in terms of physical and biological causes; 3.) that Smith intentionally hid his debt to these sources out of fear of persecution in the religiously oriented Scotland of the eighteenth century; and 4.) that Empedoclean "vortex cosmology" and "biophysics" lie at the root of the "Greek anthropology" to which Smith was drawn.

Except for one chapter which extends his assertions of classical influence to Marx and his physicalistic analyses to more modern economics, and a final unintegrated chapter which responds to an earlier reader's suggestions of French antecedents to some of Smith's classical borrowings, the book moves chronologically through Smith's major works from the juvenile "History of Astronomy" to the *Wealth of Nations*. At each stage and on each of his major theses Foley offers fascinating, suggestive, and ingenious, as well as naive and perverse ideas. He is convincing on points one and two, though he inexcusably shows almost no awareness of the roles of Hutcheson and Hume as transmitters and mediators of some of the classical influences upon Smith. Though believable, point three is claimed with insufficient support in order to explain why evidence on four is either nonexistent or indirect. With regard to point four, it seems to me that the clear ties between Smith and Plato, Lucretius, and Polybius leave the issue of Empedoclean impact not just unproven but highly suspect. Foley's argument on this point depends on the curious and unjustified claim that virtually every Greek and Roman mention of division or separation as well as every classical reference to a wheel, revolution, or circle has its ultimate source in Empedoclean vortex cosmology.

Foley's entire analysis is couched in prose so painfully awkward that it is likely to discourage all but the most dedicated reader.

RICHARD OLSON
Harvey Mudd College

ANN SUTHERLAND HARRIS and LINDA NOCHLIN. *Women Artists, 1550–1950*. New York: Alfred A. Knopf. 1977. Pp. 367. $15.00.

It is a welcome if not surprising development that women have discovered the historical creativity of their sex in the fine arts. On June 3, 1977 Claire Gibault conducted a concert of works by women composers at Chambéry, France. Earlier that year, the Los Angeles County Museum mounted the largest international exhibition of women's art ever held. Written as an accompaniment to the exhibition by its co-curators, this book includes historical essays (Ann S. Harris: pre-1800; Linda Nochlin: post-1800), biographies, and critical sty-

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listic evaluations of the works of the eighty-four artists represented.

The principal theme of the book is the gradual emancipation of women artists after the sixteenth century and their struggle to overcome the social barriers that impeded their artistic education and career goals. The paintings demonstrate the authors' denial of the existence of uniquely female stylistic traits or "of any specifically feminine tendencies whatever" (Nochlin, p. 66). Nevertheless, common experiences, especially those arising from prejudicial attitudes and traditional social roles, gave women's art certain identifiable characteristics. Before 1800 women painted mainly still-lifes, portraits, and religious scenes.

On the admission of the first woman, Catherine Duchemin, to the *Académie de Peinture* in 1663, Louis XIV declared with a flourish that talented artists would be supported "without regard to the difference of sex" (p. 34). But they could not attend drawing classes or meetings, compete for prizes, vote, or hold office. The more conservative English Academy admitted only two women prior to the twentieth century. Harris attributes the plight of women artists to the "patriarchal ideology" of the Catholic Church and to the widespread belief that women belonged in the home and had no artistic potential. Based on contemporary literature, her thesis is impressionistic and does not take sufficient account of socioeconomic conditions and relationships. Both authors are perhaps too occupied with problems women faced in attempting to crack a male-dominated society, and not enough with the broader historical framework in which they worked.

That considerable progress had been made by the eighteenth century is indicated by the fact that there were three hundred women artists by that period, compared with fewer than ten in the fifteenth century. Many earned their living by painting; some, like Rosalba Carriera, achieved fame, and none were considered odd curiosities. Nochlin considers the increased number of women's paintings exhibited in salons, the much wider range of subject matter, the campaigns for equal treatment, the development of stylistic specialization, and new employment opportunities in textile design as barometers of the progress of women artists in the nineteenth century, despite a Victorian mentality that equated professional women with socialism, free love, and atheism.

Attending the exhibition that prompted the production of this book is not a prerequisite to its enjoyment. The plates (204) render the visual aspect adequately. The introductions and biographies are written with such verve, devotion, and authority that one is readily enticed to learn more about many of these artists. This book should stimulate dissertations, monographs, and, hopefully, exhibitions of the works of individual women artists.

ROBERT M. ISHERWOOD
Vanderbilt University

TED ROBERT GURR, PETER N. GRABOSKY, and RICHARD C. HULA. *The Politics of Crime and Conflict: A Comparative History of Four Cities*. Beverly Hills: Sage Publications. 1977. Pp. xii, 792.

This long multi-authored book has much to offer, perhaps too much. The book consists of five major sections, one of which, the summary and comparative analysis by Ted Robert Gurr, is available as a separate volume, and another of which, Peter N. Grabosky's section on New South Wales (Sydney) is the condensation of another book. The other three sections deal with London, Stockholm, and Calcutta. All four cities under scrutiny are covered from the pre-uniformed police era to the present. The book leaves the reader stunned with detail, holding a model to explain civil strife in one hand and a question mark about crime in the other. And while one might wish that the book were tidier, shorter, and less filled with narrative detail, one must admire it for being the only book of its sort. Its shortcomings are those to be expected of a bold effort at a model-building, historical, comparative, quantitative, and qualitative study of four cities, one distinctly non-Western, over a century and a half time span.

Although never clearly emphasized, the book's main concern is with the relationship of civil strife or rebellion to social structure and change. Crime enters the analysis because most social disorder is defined as criminal and because the criminal justice system constitutes the main social response to both forms of behavior. Disappointingly, the authors never work out the explicit relationship between crime and civil strife—unusual in a book which is careful and explicit about its methods, assumptions, definitions, and conceptualizations. In an intriguing remark, Richard C. Hula, in the section on Calcutta, notes that while civil strife confronts the status quo, criminal behavior seeks to avoid it (p. 450). And in his theoretical model Gurr differentiates between the results the two behaviors tend to produce, but he never explicitly analyzes these critical differences.

Three sets of multiple regression tables statistically summarize the empirical relationships discussed in the book, most of which is nonquantitative or presented in clear graphical form. London, Stockholm, and Sydney each have a regression table; the authors excluded Calcutta because of the poor data available. There is no evidence that the authors tried to make the tables comparable

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across the three cities: to a degree this is understandable, as finding annual data for indicators on even one city is, in itself, difficult. But, when the regression for Stockholm explicitly excludes a measure of police strength (p. 309) while the regression for Sydney includes the same variable (p. 451), one wishes the authors had cooperated more closely. Not only do the regressions use different variables, but one (Sydney, p. 451) uses more than the other two, and of the remaining two, each uses a different computational technique (London, pp. 208–11; Stockholm, pp. 310–11). And of the fourteen regressions for Stockholm, twelve violate the assumption of uncorrelated error terms, rendering the results ambiguous and difficult to evaluate.

Given these problems with the statistical analysis, it is fortunate that the principal author relies upon graphically presented data for his larger, concluding analysis of the three Western cities. And given the massive welter of material he had to work with, the final 150 pages by Gurr come across as a tour de force; unfortunately, to summarize his comments one must distort. Gurr shows how indicators of crime rates for the three cities decreased through the nineteenth century, levelled off in the first third of the twentieth century, and then began the phenomenal increase with which we are all familiar. Gurr distinguishes two causal eras, the first in the nineteenth century when policing worked and offense rates declined. In the second twentieth century era, policing no longer drives offenses down, partially, he suggests, because of expanding criminal opportunities and because victims are no longer so fearful of reporting offenses. And thankfully, Gurr spares us dire predictions about the evils of urbanization or other vaguely conceptualized social forces.

Contrary to the trends of individual criminal offenses, group crimes of rebellion have decreased in the three Western cities and risen in Calcutta. Gurr finds little in this pattern to support Charles Tilly's model of the evolution of collective violence, instead asserting that the Western decline resulted from elite accommodation to mass demands. In Calcutta, on the other hand, rebellion has met and continues to meet with oppression and nonaccommodation, and it has become endemic. (One wonders—does this mean that the oppression simply has been ineffective, or is there a law-like relationship present?)

Gurr's analysis of collective violence is inherently more satisfying than his analysis of changing criminal offense patterns: the problem is not Gurr's alone, for without doubt this monumental book represents one of the best historical analyses of crime to date. The problem, I suspect, lies at the conceptual level; crimes of rebellion are mass acts directly challenging the state, and both sets of actors can at least agree about that. Individual criminal offenses, on the other hand, are very seldom perceived by the offenders as being direct challenges to the state, while to the state the essence of the criminal act is its threat to the legitimate power of the state. It is little wonder then, that the mode of analysis appropriate for dealing with rebellion falters when confronted with "ordinary" crime. Nevertheless, one can only praise and applaud these authors' efforts to raise the study of urban crime and repression from particularism to cross-national, theory-directed research.

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WILBUR R. MILLER. *Cops and Bobbies: Police Authority in New York and London, 1830–1870*. Chicago: University of Chicago Press. 1977. Pp. xii, 233. $16.00.

Why does London's impersonal, restrained police force of bureaucratic professionals stand in such sharp contrast to New York's personal, often violent department of democratic "amateurs"? Or, to paraphrase the author at another point, why did two organizations which shared the same goals of crime and class control and order maintenance, and which emerged in countries which enjoyed close cultural and legal ties, develop such dramatic differences in *style* (especially since New York consciously borrowed from the London model)? The author's comparative approach allows him to suggest reasons for divergences with greater authority than he chooses to exercise. Wilbur R. Miller is an artist constructing a collage of historical causality rather than a scientist conducting a controlled experiment in the laboratory of the past. Thus, crucial decisions made (or not made) by contemporary commanders to shape images best suited to win legitimacy for their forces played an important part. At the same time the two departments responded to different social, institutional, and, particularly, political situations. Miller's collage captures the complexity of interaction between conscious decision and social-political forces; it is a formidable work of art.

Miller examines many points of distinction between the two institutions, including definition of mission, organization, recruitment, training, and discipline, and he measures the manner and effectiveness of police performance with imagination and skill. His analysis of the relationship of the police to the legal, judicial, and political systems is expertly done, as is his demonstration of how a heterogeneous public responded to changing police strategies. His case study of the effect on police of evangelical efforts to control the morals of citi-

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zens provides a vivid illustration of how the police reacted to public pressure. Miller brings an impressive scholarship to bear on these topics, including research in published and unpublished official records, newspapers, manuscripts, pamphlets, music-hall songs, and books of the period, as well as a good command of modern histories (he makes effective use of James Richardson's solid study of the New York police) and sociological studies of police, crime, law, and social conditions. One especially fine example of the author's ability to draw upon all of these sources can be found in his examination of the relationship between Irish police and the working class in New York (pp. 153–55). Miller generally is at his best in comparing belief and behavior, perception and practice. Using a variety of statistical evidence (better for London than New York), such as the distribution of police by area, the number of physical assaults on officers, and time-series of committal-arrest and conviction-arrest ratios, he critically examines contemporary complaints and claims regarding police activity. Even in the most technical places, the book remains eminently readable. It is an excellent work that should be read and praised widely.

EUGENE J. WATTS
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ROGER DINGMAN. *Power in the Pacific: The Origins of Naval Arms Limitation, 1914–1922*. Chicago: University of Chicago Press. 1976. Pp. xiii, 318. $19.00.

The period of the First World War witnessed such an epidemic of naval expansionism that the achievement of disarmament among Great Britain, the United States, and Japan three years after Versailles was a truly remarkable phenomenon. In a stimulating tri-national study of the prelude to the Washington Conference, Roger Dingman addresses the problem of why an agreement on limitation was reached.

Traditional historians would attribute the decision to disarm to the fact that the concerned nations reached a mutual understanding of their respective hegemonic interests and strategic and economic capabilities. Dingman breaks new ground by highlighting the role of domestic political factors in molding policies of expansion and limitation. Japan's postwar premier Hara Kei, for instance, is pictured as initially catering to big-navy advocates by pressing for an eight-battleship, eight-battlecruiser fleet. But by 1921 the "politics of national defense" had shifted so that Hara gained more political mileage by heeding the demands of cost-conscious Dietmen and business magnates and by reasserting civilian restraint upon the admiralty. Presidents Wilson and Harding and Prime Minister Lloyd George were similarly driven by the need to cultivate favor at home. Students of the World War I period and the problem of arms limitation will welcome Dingman's new approach to the determinants of policy, but not all will go so far as to affirm his blunt assertion that "politics within the capitals" carried far more weight than "international relationships among them" in the evolution of the disarmament scheme (preface). Nonetheless, all will respect the author's rare combination of openness to the theoretical insights of contemporary social scientists and diligence in documentary research. The book reflects painstaking use of naval files, cabinet records, personal manuscripts, and interviews in London and Washington with proportionate attention to Japanese-language archival materials.

A few secondary deficiencies bear mention. The role of the Paris Peace Conference in establishing Japanese acquiescence to the Anglo-American order and the principle of arms limitation deserves more emphasis, for Japanese naval documents show disarmament preplanning in the Navy Ministry as early as August 1920. The author's propensity for rhetorical imagery and tidy comparisons leads occasionally to uncritical statements. Generalizations concerning the growth of naval budgets (p. 6) are not supported by the data in the accompanying chart. The book's title is a partial misnomer, for the content of this work will serve to establish Roger Dingman as a formidable historical interpreter of power in the Atlantic as well as power in the Pacific.

THOMAS W. BURKMAN
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MYRON J. SMITH, JR. *World War II at Sea: A Bibliography of Sources in English*. In three volumes. Metuchen, N.J.: Scarecrow Press. 1976. Pp. 337; 427; 586. $35.00 the set.

Another prolonged "conventional" war of the nature of the Second World War appears increasingly unlikely, and probably even more remote would be a repetition of the giant surface naval operations in that conflict, ranging over thousands of square miles, and involving hundreds of warships. Yet that global struggle, the greatest endeavor in human history, continues to fascinate succeeding generations of historians and laypersons.

Myron J. Smith, Jr. is to be praised for his indefatigable labors in compiling this bibliography of English-language works on the Second World War at sea. The three-volume effort containing more than 10,000 entries represents the culmina-

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tion of the author's exhaustive *American Naval Bibliography* series (5 vols., 1972–74), covering the years 1770–1941. As such, it is a worthy companion to Robin Higham's more selective *Guide to the Sources of United States Military History*.

The entries are divided not only into theaters of operation, campaigns, battles, and ships, but also into production, technology, the home front(s), intelligence, logistics, research and development, and a grab-bag of miscellanea and trivia.

The sources are truly catholic, ranging from the United States Naval Institute *Proceedings* and the *Journal* of the Royal United Service Institute, through *Canadian Machinery, Independent Woman,* the *Indiana Telephone News,* and the *Scottish Geographical Magazine*. Smith also includes oral history materials, doctoral and masters dissertations, "cruise books," squadron histories, Japanese monographs, and "in house" official efforts, published and unpublished, of wildly varying quality.

Obviously, with such a range and breadth of entries there could be little evaluation of each entry, and the author confines himself for the most part to simple description. This lack of evaluation and selectivity may be considered a failing, but one easy to understand and forgive.

The Scarecrow Press and the author are to be congratulated for permitting very few typographical errors to escape them, and for providing an attractive format and durable binding, factors to be considered in the light of today's book prices. Readers will also appreciate the thorough author and name indexes and the inclusion of a naval chronology.

On the debit side, the author's slangy comments on occasion strike an incongruous note. And there are a few questionable assertions, such as that "medical science during this period began to make use of psychology and psychiatry in handling military personnel." On the contrary, their use dates back at least to World War I. Such lapses are rare, but what is the intelligent reader to make of the following comment by E. M. Eller, Rear Admiral, U.S. Navy (Ret.) in a preface in volume three (p. 253): "More Americans need to understand what maritime power has meant to our country—and to the spread of freedom and Christianity, which sail or sink together." This geopolitical mysticism might read strangely in landlocked but free Switzerland, or in non-Christian but free and maritime Japan. And is not the atheistic and unfree Soviet Union now a major sea power? Admiral Eller was Director of (U.S.) Naval History for over fourteen years.

But the above *obiter dicta* aside, Smith has fulfilled his stated ambition to achieve "the most comprehensive bibliography on any single aspect of the Second World War ever published in the United States." Historians and researchers interested in the Second World War or in maritime affairs, librarians, and students from the secondary level to graduate school will remain indebted to Smith for many years.

STANLEY SANDLER
Fairfax, Virginia

ALVA MYRDAL. *The Game of Disarmament: How the United States and Russia Run the Arms Race*. New York: Pantheon Books. 1977. Pp. xxvi, 397. $15.00.

Alva Myrdal's long and distinguished career has included, in recent years, some twelve years as Swedish representative in the United Nations Disarmament Committee. There she made herself a vigorous and respected spokesperson for the nonaligned nations and a passionate voice for genuine and far-reaching disarmament. But, whatever the passion and the ethical concerns, her latest book abounds in hard information and sober analysis. She is strikingly well informed on a wide range of technical subjects, from strategic doctrine to scientific forecasts. To this factual base she adds what, she correctly stresses, has so often been absent from other treatments of the arms race: the element of morality, perceived from a global, international perspective.

Myrdal's approach is well suggested by her assertion that, while "our world is hovering at the edge of an abyss," in fact "the superpowers have not seriously tried to achieve disarmament" (p. xxiv). Though she assigns some responsibility to all the parties concerned, the primary blame for the present situation goes properly to the U.S. and the USSR. As she sees it, they have tacitly and unwittingly conspired to frustrate genuine arms limitations and reductions. They have done so, in part, out of doctrinal misconceptions (focusing on the search for an illusory parity in offensive forces instead of settling for invulnerable second-strike capability to inflict unacceptable damage on a potential enemy); in part, thanks to the role of special interests, notably the military, business, labor, scientists, and politicians.

The books amounts to a systematic and well-argued examination, first, of the anatomy of "unreason," and second, of the record of disarmament debates and negotiations since World War II. The essential facts have of course been known—in fact, they are available elsewhere in greater detail—but they are impressively marshaled here, albeit to make a point. It is a point too important to be dismissed as naive, as has all too frequently been done by American as well as Soviet policy-makers and political observers.

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In the course of her survey, Alva Myrdal examines technological "improvements" in weapons systems, nuclear proliferation, test bans, the outlawing of particularly "cruel" weapons, and the problems of verification. On a number of topics she does indeed have fresh insights to contribute—for instance, on the prospects of proliferation, on U.S.–West European differences of outlook, on the illusion of greater security by means of more and "better" weapons, on the historical record of missed opportunities, and on the social costs of the "weapons culture" in supposedly advanced societies.

At the same time, there is much to query and to examine further in these pages. Even a sympathetic reader may feel that she makes too light of the political uses of strategic power (real or imaginary, no matter), of the desirability of stabilizing the military environment even short of drastic reductions, and of the elements of rationality in the current posture of the major powers. In this reader's view she is also too sweepingly negative with regard to the Strategic Arms Limitation Talks, and perhaps too willing to give the People's Republic of China the benefit of every doubt. While she realistically dismisses general and complete disarmament as utopian, she offers instead a thoughtful and elaborate agenda of proposed measures which—while undoubtedly urgent and desirable—is not likely to find many takers in either camp.

And yet, this is the product of a rare combinaton of an excellent mind, a lifetime of wisdom, and a passion for human decency and good sense, that deserves a wide audience. It may have more to say to analysts of current affairs than to historians strictly speaking. But it raises troublesome questions which even those who disagree must want to come to grips with.

ALEXANDER DALLIN
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ANCIENT

P. A. BRUNT, translator. *Arrian.* Volume I, *Anabasis Alexandri*: *Books I-IV.* (Loeb Classical Library.) Cambridge, Mass.: Harvard University Press. 1976. Pp. lxxxvi, 547. $7.00.

The publication of the first volume (Books I–IV) of P. A. Brunt's translation, with its introduction, commentary, and appendixes, is a major event in the historical study of Alexander. The older translation of Arrian's *Anabasis* in the Loeb series, by E. Iliffe Robson, was notoriously inaccurate; and although Brunt offers a revision of this rather than an entirely new rendering, the result is extremely successful, lucid, and faithful to the original. The discussion of historical problems in the introduction, commentary, and appendixes is thorough in its coverage, carefully grounded in the ancient sources, and judicious in its judgment. The introduction places the narrative of the *Anabasis* in its historical setting, examining at length the sources for Alexander's career and the historical background to his achievement, with sections on the earlier history of Macedonia, the Persian Empire, and the army of Alexander. Individual historical problems raised by the text are dealt with briefly in the commentary or at length in a series of detailed appendixes. There is no attempt to view the *Anabasis* as a literary work; the concern throughout is with its value as a historical source for the career of Alexander. As such, apart from its intrinsic value, Brunt's volume serves as an admirable summary of the current portrait of Alexander in historical scholarship.

For the modern historian not the least of the forces unleashed by the great Macedonian is the copious fount of historiography dealing with Alexander. In our own day, as in antiquity, interest in Alexander has been cyclical. Arrian himself wrote amidst a major revival of interest in the figure of Alexander, who, through the alchemy of imperial propaganda, was transmuted into the prototype of the Antonine emperor, god-sent savior and benefactor of an ecumenical empire, who like a new Hercules labored unceasingly on behalf of mankind. Under more ambiguous circumstances and with less ennobling results, the last decade (1965–75) has witnessed a veritable flood tide of scholarly interest in Alexander, one which shows no sign of abating.

The scholarly decade began with the devotion of an entire issue of *Greece and Rome* (12[1965]) to Alexander the Great and closed with the convocation of a Fondation Hardt colloquium on Alexander (*Alexandre le Grand*, ed. E. Badian, *Entretiens sur l'antiquité classique*, 22 [1976]). That decade witnessed the publication of at least nine scholarly biographies of Alexander. The list includes biographies in English by J. Snyder (1966), R. D. Milns (1968), J. Hamilton (1973), R. Fox (1973), and P. Green (1974). C. B. Welles' *Alexander the Great and the Hellenistic World* (1970) might also be cited in this context. F. Schachermeyr's *Alexander der Grosse, das Problem seiner Persönlichkeit und seines Wirkens* (1973) is a monumental achievement. G. Wirth's *Alexander der Grosse in Selbstzeugnissen und Bilddokumenten* is an excellent brief biography for the general reader, comparable to P. Briant's *Alexandre le Grand* (1974). Some older biographies have been reissued in revised format, and several biographies by nonacademics have also appeared; of

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these M. Renault, *The Nature of Alexander* (1975) is much the best. Of the plethora of recent scholarly biographies, Schachermeyr's may be characterized as the most significant, Hamilton's as the best balanced, Green's as the best written, Miln's as the least successful, and Fox's as the most controversial.

The journals bore equal witness to a marked interest in Alexander; and the preparation of bibliographies of scholarly work on Alexander became a major industry, spawning three such studies in a three year period (1970–72). N. Burich, *Alexander the Great. A Bibliography* (1974) is an uncritical list of titles; E. Badian, "Alexander the Great, 1948–67," *Classical World*, 65 (1971): 37–56, 77–83, is a model critical bibliography; and J. Seibert, *Alexander der Grosse* (1972) is arranged along the lines of a handbook, a skeletal biographical account serving as the framework for a complete bibliography of modern scholarship. *Alexander the Great: The Main Problems* (1966), edited by G. T. Griffith, gathered together seminal articles of the preceding period and provided an overall definition of the field. Finally, the modern historiography of Alexander became the object of serious scholarly attention. A. Demander, "Politische Aspekte im Alexanderbild der Neuzeit," *Archiv für Kulturgeschichte*, 54 (1972): 325–63, is hopefully the harbinger of more such studies, attempting to place modern historians of Alexander within the context of their contemporary political and cultural settings and best undertaken by those who are not specialists on Alexander and who are thus removed from the blindly polemical internecine strife which his figure seems to provoke.

The hallmark of the decade has been increased consensus on an approach to Alexander and a portrait of the man which differs fundamentally from the romantic and idealistic vision current in much of the scholarship of a generation ago. Arising in reaction to the provocative images of Alexander presented in W. W. Tarn's *Alexander the Great* (1948) and F. Schachermeyr, *Alexander der Grosse. Ingenium und Macht* (1949), the aim of much recent scholarship has been to historicize Alexander, to see him as a man of his own time and to interpret his actions and motives solely in the light of the sources. An early and significant critic of Tarn and Schachermeyr was R. Andreotti, "Il problema di Alessandro Magno nella storiografia del ultimo decennio," *Historia*, 1 (1950): 583–600. Equally seminal was H. Strasburger's critique of Tarn's methods of source criticism (in his review of Tarn in *Bibliotheca Orientalis*, 9 [1952]: 202–11 and in detailed studies like his "Alexanders Zug durch die gedrosische Wüste," *Hermes*, 80 [1952]: 456–93). F. Hampl, "Alexander der Grosse und die Beurteilung geschichtlicher Persönlichkeiten in modernen Historiographie," *La nouvelle Clio*, 6 (1954): 91–136 presented a balanced critique of Tarn and Schachermeyr. Building upon these foundations, the most influential figure in the Alexander historiography of the last decade has been E. Badian. He began his scholarly contributions with attacks upon Tarn's methodology ("The Eunuch Bagoas," *Classical Quarterly*, 8 [1958]: 144–57) and on Tarn's central theme of Alexander's vision of the unity of mankind ("Alexander the Great and the Unity of Mankind," *Historia*, 7 [1958]: 425–44). These were followed by treatments of Alexander's accession ("The Death of Philip," *Phoenix* 17 [1963]: 244–50), the affair of Philotas ("The Death of Parmenio," *Transactions of the American Philological Association*, 91 [1960]: 324–38) and of Alexander after the return from India ("Harpalus," *Journal of Hellenic Studies*, 81 [1961]: 16–43). Together these presented a composite picture of Alexander vastly different from that of Tarn. A list of Badian's contributions to the study of Alexander appears in the *Protocol of the Twenty-first Colloquy of the Center for Hermeneutical Studies in Hellenistic and Modern Culture*, Berkeley, California, March 7 (1976): 74–75. A collection of Badian's contributions, published in German translation by Teubner, has been announced.

The result of this work has been a diminished portrait of Alexander. Gone is Tarn's vision of Alexander as one of the supreme fertilizing forces of history, a dreamer following his vision of the brotherhood of mankind. Alexander remains "a dazzling phenomenon, but one terrifying rather than attractive" (C. B. Welles, "Alexander's Historical Achievement," *Greece and Rome*, 12 [1965]: 228). It is as a soldier and conqueror, not as a visionary, that we must judge him. It is alongside figures like Caesar and Hitler that he must take his historical place, distinguished neither by the former's universal genius nor the latter's taint of pure evil. True, his bloody genius began a new historical epoch, but it was one of misery and unceasing war. Alexander's character, too, has become less attractive. An increasingly neurotic young man, he schemed and murdered his way in pursuit of the secure tenure of absolute power only to find himself at last with no use for this power and security unattainable. (Cf. Badian's observations on Alexander's character and achievement in his essay "Alexander the Great and the Loneliness of Power," *AUMLA* [1962]: 80–91, reprinted in *Studies in Greek and Roman History* [1964]: 192–205 and in his survey of scholarship in *Classical World*, 65 [1971]: 46.)

Unfortunately, aspects of this new portrait are threatening to pass into scholarly dogma; and what began as a healthy re-evaluation, carefully grounded in the sources, has in less competent

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hands degenerated into the most puerile sort of historical debunking. Among large-scale treatments this is most true of R. D. Milns' unfortunate *Alexander the Great*.

The inevitable reaction has already begun; and recent attacks have been launched against the view of Alexander as a youthful parricide and an early middle-aged debauched megalomaniac. The most extensive is K. Kraft's posthumously published *Der 'rationale' Alexander*, ed. H. Gesche (1971). More minor caveats against certain aspects of the now fashionable portrait of Alexander include A. Bosworth, "Philip II and Upper Macedonia," *Classical Quarterly* 65 (1971): 93–105 and J. Fears, "Pausanias, the Assassin of Philip II," *Athenaeum*, 53 (1975): 111–35, doubting Alexander's complicity in his father's murder; Bosworth, "The Death of Alexander the Great, Rumour and Propaganda," *Classical Quarterly* 65 (1971): 123–27 (the last year of Alexander's life); Fears in *Protocol of the Twenty-first Colloquy* (cited above), 23–31 (Alexander's deification); C. G. Thomas, "Alexander the Great and the Unity of Mankind," *Classical Journal*, 63 (1968): 258–60; P. M. Fraser, *Ptolemaic Alexandria* (1972) II: 777, 1114 (Alexander and the Peripatetics).

The cliché that each generation must write its own history is nowhere more dubious than in regard to a topic like Alexander the Great in which no real accretion to our sources has occurred. Compared to other areas of ancient history, the ancillary disciplines like epigraphy and numismatics have offered little towards an elucidation of Alexander's career. The study of Alexander is the study of the literary sources; *Alexanderforschung* must be *Quellenforschung*. The modern study of Alexander begins not with Droysen but much earlier, with the critical appreciation of Arrian as the primary source from which any treatment of Alexander must be drawn, an attitude which characterizes the excellent account in the anonymous *Universal History* published in London in 1747. Thus it is entirely proper that a critical re-evaluation of Arrian has been the fulcrum of the revised and diminished portrait of Alexander which now dominates current scholarship. In fact the diminution of Alexander's status has rather sharply paralleled the fortunes of his enthusiastic admirer, Arrian. In this view, Arrian's main sources, Ptolemy and Aristobulus, appear as a pair of apologists, not unprepared to disguise the unfavorable side of Alexander's character and actions and not immune to the temptation to write their narratives under the impress of personal animosities and the political history of the post-Alexander period. Brunt's appreciation of Arrian (p. xxxiv) is characteristic. "He was a simple honest soul but no historian." His account, Brunt continues, is more detailed and coherent and less concerned with the trivial and fabulous than those of Q. Curtius Rufus and Diodorus of Sicily. However, for Brunt, as Arrian's style is less brilliant than that of Curtius, so his judgment is more naive; and he was incapable of realizing that, valuable as they were, the accounts of Ptolemy and Aristobulus might be marred by tendentiousness.

With this perspective of Arrian, it is not surprising that the aim of much recent scholarship has been to supplement and to correct Arrian with material found in our other main sources, Curtius, Diodorus, Plutarch, Trogus-Justin, and the extant fragments of other historians. The results have been uneven, and at times there has been a tendency to violate fundamental canons of source criticism, conflating sources of unequal value and citing as historical material found in rhetorical speeches inserted by the ancient historian into his narrative. The dangers inherent in this method are nowhere more apparent than in the piecemeal use of Curtius, and such uncritical employment of Curtius has given rise to a plethora of red herrings to bedevil researchers in our field.

Skepticism about the now fashionable rehabilitation of Curtius may be warranted by a more fundamental consideration. Our preconceptions of Alexander continue to shape our attitudes toward the sources. For obvious reasons, Tarn found Arrian a sympathetic authority. Curtius' portrait of a young man whose very success corrupted him into a debauched, murdering, megalomaniacal tyrant is ominously similar to the image of Alexander which now dominates much current scholarship.

At the very least such attempts critically to re-evaluate Arrian in the light of Curtius are premature until we possess a deeper and more sensitive appreciation of the *Anabasis* and the *Historiae Alexandri Magnis* as literary works. In approaching Thucydides and Tacitus, the modern historian is guided by a long and distinguished tradition of literary criticism, viewing the works not as historical source books but as artistic compositions. Such a tradition is almost entirely lacking in our field. The study of Arrian and Curtius has been the preserve of historians, whose sole and perfectly laudable aim is to extract historical data. The most pressing desideratum is for a detailed critical appraisal of the *Anabasis* and the *Historiae Alexandri Magnis* from the literary perspective. To take a single example, Curtius' treatment of Bagoas becomes a valid piece of historical evidence only within the context of the larger composition of the *Historiae*, specifically Curtius' use of figures like Bogoas and Abdalonymus as pegs on which to hang his characterization of Alexander as good king transformed into tyrant under the weight of good fortune. Indeed, the entire problem of the

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"vulgate" tradition is a literary as well as historical problem. It is not enough endlessly to debate whether Cleitarchus is the original source for the parallels in Diodorus, Curtius, and Trogus-Justin. In the centuries between Cleitarchus and our own main sources the image of Alexander was not static. It was constantly evolving, subjected to what Eliade calls "the corrosive action of mythicization."

The vulgate image of Alexander was a popular or folk image; its development comparable in many respects to that we observe with heroes in epic poetry. Popular memory is not concerned with historical event but with myth. It finds difficulty in retaining individual events and real figures, for it functions by structures different from those of academic history: categories instead of events, archetypes instead of historical personages. As in other areas of ancient history, it is at his own peril that the student of Alexander excludes himself from relevant developments in disciplines like comparative literature and folklore. In short, we must be more willing to see Arrian and Curtius as literary figures in their own right. As part of this the determination of Curtius' date remains a critical problem, for only with some idea of the circumstances of its composition can we adequately come to terms with the extraordinarily complex portrait of Alexander in the *Historiae*. Arrian, by contrast, presents us with a well-defined personage and a large corpus of writings. Tendentiousness may have a source other than Arrian's authorities; and more consideration is due Arrian and the epoch in which he wrote as formative elements in his portrait of Alexander. (G. Wirth, "Arrian und Traian," *Studii Clasice*, 16 [1974]:169–209 is a salutary step in this direction.) The *Anabasis* must be seen within the context of Dio Chrysostom's second oration on kingship and Plutarch's *De Alexandri Magni fortuna aut virtute*, primary documents in that revival of interest in Alexander so significant in the imperial propaganda of the late Flavian and Antonine periods.

A literary appreciation of Arrian, Curtius, and the vulgate tradition will not carry us back to Alexander the man; but it is essential to understanding the process by which our image of the historical Alexander evolved. The man and his motives are irretrievable. Surveying scholarship on Alexander from 1948–67, Badian suggests that at last, "We are perhaps beginning to emerge from the Alexander Romance." The hope is an illusion; and the aim of much recent scholarship to historicize Alexander is laudable but misconceived. We can never emerge from the Alexander Romance; and as with Jesus of Nazareth, attempts to historicize Alexander must ultimately fail. The process of mythicization began in the king's own lifetime; and our sources are so tainted by it that we can never reconstruct *wie es eigentlich gewesen ist*.

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R. A. MARKUS. *Christianity in the Roman World*. New York: Charles Scribner's Sons. 1974. Pp. 192. $10.00.

R. A. Markus tells us he intends to "trace the history of Christian self-awareness in the Roman world, the ways in which Christians saw themselves as a distinct group in society, the ways in which they identified themselves with it—its structures, its values, its culture—and the ways in which they opposed themselves to or differentiated themselves from it."

He means it. The church-world problematic rigorously focuses and limits the material through nine chapters, spanning the time from the church's beginnings as a Jewish "sect among sects" (chapter 1), through its transformation into an imperial Church (chapter 5), and extending to the parting of ways between Eastern and Western Churches, when barbarian kingdoms replaced empire as the "world" with which the Western Church would have to deal (chapter 9).

Each critical moment of the Church's struggle for self-definition as a social group is explored within a carefully drawn context of Roman life: e.g., the movement's sense of being a "third race" over against the pagan world; its emerging intolerance and ambition to become a universal cult; the indissoluble process of institutional and doctrinal self-definition; the recurring conflict (personified by Clement of Alexandria and Tertullian) over the appropriation of learned culture; the parallel—and for the West paradigmatic—ecclesiological struggle between Donatism and the imperial Church; Augustine's pivotal resolution of these cultural and political issues; the leadership of Gregory I on the brink of the Church's domination of medieval society.

Markus' achievement is stunning. In fewer than two hundred pages, by sticking to his theme and refusing to indulge the temptation to tell everything, he has produced a masterpiece of selection. This lean book is never overburdened with data; yet it is studded with illuminating examples and reliable summary statements, and rich in memorable generalizations. Especially helpful is the deft handling of the confusing but epochal transition from Augustine to Gregory I.

The interpretive slant, the most original part of the work, derives from the author's study of the

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Church's flirtation with imperial theology and the Augustinian antidote (*Saeculum: History and Society in the Theology of St. Augustine* [1970]). Especially gripping on this score is Markus' portrayal of two theologians who surpassed their rival parties—Augustine and Tyconius. The one could not remain quite imperial; the other could not be perfectly donatistic. Each recognized, in the end, the futility of his triumphalist or sectarian pretensions.

But "world" in this book goes beyond the political dimension. Seventy-four illustrations give not merely cosmetic but substantive dimension to the text, which also contains some apt comment on the emergence of Constantinian church architecture. The book concludes with brief bibliographical essays for each chapter.

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ALAN CAMERON. *Circus Factions: Blues and Greens at Rome and Byzantium*. New York: Oxford University Press. 1976. Pp. x, 364. $34.75.

The circus factions are among the most widely-known of Byzantine institutions, characterized by their participation in events such as the notorious Nika revolt, in which 30,000 persons are supposed to have perished. Modern scholars, convinced that something more substantial than simple rivalry at the chariot races must have underlain factional violence, have variously identified the factions as social or economic groups or as an urban militia; their frequent clashes have been seen as thinly-veiled social, religious, or political conflicts in which the factions represented the survival of democratic sentiment in the autocratic world of early Byzantium.

Alan Cameron's *Circus Factions,* a companion to his *Porphyrius the Charioteer* (1973), is a long-awaited analysis of this important phenomenon. The first half of the book, as Cameron admits, is largely negative, and devoted to a demolition of the previous interpretations of the factions and their place in Roman and Byzantine history. Much of this argument is detailed and somewhat difficult for the nonspecialist, concerned as it must be with the elucidation of specific texts and the analysis of terminology. In the end, Cameron manages to separate the factions from other groups (for example, the "people" as a whole and the so-called "demes," who he denies ever existed) and to identify them simply as the partisans of one racing color or another.

With this work behind him, Cameron moves on the more positive half of the book. He starts from one basic assumption: that at the beginning the activities of the factions were always connected with the hippodrome, the place where the Roman emperor came to be seen and praised by his subjects. In the early empire, however, the factions were not particularly troublesome, nor were they widespread in the Greek East, a situation in marked contrast with that of the early Byzantine period when most popular disturbances were connected with the factions. The cause of this change was not that the factions became "politicized," but that they were amalgamated with the theater claques who had always been a focus of urban unrest. The unfortunate side of this was that the rowdiness which characterized the theater claques now found an outlet in the hippodrome, but the violent activities of the factions should not obscure the more significant result of the reform: the factions, always clearly identifiable in their colored dress and individual stations, could now be expected to play an increasingly important and reasonably reliable role in imperial ceremony. As the centuries passed, this role became dominant, although there were probably "circus riots" even in the middle Byzantine period. This growing importance of the factions, rather than any official suppression, should explain their "decline": "They were not prevented from rioting . . . they simply became too grand for it."

Cameron's central thesis will be discussed for decades, and it will certainly be unpopular, since it deprives Byzantinists of one of their favorite illusions, the existence of Byzantine *partis populaires*. Nevertheless, Cameron has argued his case convincingly, and in broad outline there is no doubt he is correct. In addition, he has presented the evidence fully and even his severest critic must admit that he has raised the discussion to a new level. The issue, of course, is a notoriously difficult one, but this reviewer wishes that Cameron had discussed the amalgamation of the factions and the theater claques a little more fully, particularly in light of the slightly different explanation of the escalation of factional violence given in *Porphyrius*. When, exactly, did the amalgamation take place? (Apparently it was some time during the fifth century.) Was it the result of a single administrative act or did it occur more slowly over time? Why are the sources silent about such a change? (No argument *a silentio* would be valid here and Cameron's circumstantial argument is convincing, but it is peculiar that there is no more direct evidence.)

Circus Factions represents an impressive display of scholarship and a command of a wide variety of materials from the dim recesses of the Roman monarchy on through to the tenth century and, as Cameron himself points out, it is the author's fa-

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miliarity with both the Roman and the Byzantine background of the factions that has made this analysis possible. Nevertheless, the book is important not only to the student of these periods, but to anyone interested in social history and the phenomena of violence and crowd behavior. Cameron's comments in these areas are terse and conservative, but he has presented a body of reliable material which should serve as a significant resource for theoreticians in a number of disciplines. At its best, this book is an important prolegomenon to the study of early Byzantine social history, and Cameron's methods, his organization, his command of language, and his good historical sense should be pointed out to students as models for emulation.

TIMOTHY E. GREGORY
Ohio State University

MEDIEVAL

DONALD MATTHEW. *The Medieval European Community*. New York: St. Martin's Press. 1977. Pp. 515. $22.95.

One may slice into the same historical period from many different directions, and no two of them will reveal identical planes. Donald Matthew's plane across the period 1100–1500 offers little in the way of conventional chronology or narrative history but a great deal in the way of the analysis of mental outlooks, perceptions of human nature and society, and emphasis upon the unique historicity of the period, an insistence upon understanding the past on its own terms. One of the virtues of this book, which is not an easy one for those without some general familiarity with medieval European history, is its insistence upon understanding medieval terms of reference and its resolutely analytic approach to all phases of human activity in support of its main thesis. This thesis, neither wholly new nor likely to be universally accepted, states that medieval Europe consisted of a single community. In spite of its many particularistic material and social circumstances, the often limited horizons of its members, and its limited resources, "Europe created out of many local, traditional communities, a single society, superficially divided amongst quarrelsome princes, but fundamentally united for the purpose of enabling creative individuals to realize their potential for the common good." It is another virtue of the book that Matthew's "creative individuals" are not usually the traditional great men and women of historical literature, but adventurous thinkers, entrepreneurs, craftsmen, and rulers considered as often collectively as individually. Matthew often subtly plays off the contrast between modernist presuppositions about human nature and society (see his stimulating treatment of *societas*, p. 119) and medieval views.

The first chapter, and in some ways the least successful, surveys in fifty pages the geographical and regional character of Europe and the general chronology of its history between 1100 and 1500. Such an approach enables Matthew to say many important and effective things about the importance of water and water routes in the Middle Ages, to treat Northern Europe rather better than it is generally treated in surveys, and to lay out his major theme, the overriding cultural and societal unity of medieval Europe, without resorting to anachronistic political analysis or overloading the images of universal authority—pope and emperor—who do not really enter the book until much later. On the other hand, this chapter also reflects the difficulties of such an approach, taxing the uninformed reader with such accurate, but virtually incomprehensible sentences as this: "Early in the thirteenth century the house of Barcelona which had been deeply involved in the feudal politics of this region surrendered its interests there to the nominal suzerain, the king of France, as a consequence of the Albigensian crusade against Provençal heretics" (p. 29). It is the chronology in this book that suffers most.

Chapter two, "Setting the Records in their Historical Context," is a study of the mentality of the masculine world of traditional Europe and the problems its ontology necessarily poses to modern readers. This chapter is far more an analysis of the minds behind the records than of the nature of the records themselves. This is a useful and intriguing chapter, particularly since the character of different types of records is so essential in understanding the weight the historian must give them. The nature and extent of literacy, education, ideas of history (and their bearing upon the chronicle and annal)—with exemplary use of the life of Cola di Rienzo (pp. 84–86)—legal relationships and education, and the arts are all analyzed, usually effectively. Matthew's focus here and elsewhere is less upon the "history" which the records reveal than upon the historicity of their production and their epistemological character. Chapter three analyzes social theory, rural life, trade, finance, towns, and crafts and industry. Here and throughout the book Matthew cites directly from individual texts, indicating the breadth of his research and strengthening his generalizations. Chapter four, "The City of God," strongly emphasizes the social role of Christian belief in the barbarian world as a dynamic force in European society and culture. He then deals in turn with monastic

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society and reform, doctrinal and legal disciplines, the papacy, the mendicant orders, pastoralism (much better treated here than in most general surveys), the universities, and the idea of Christendom. The remaining chapters deal with "Lay Dominion," "The Communities under Strain," and "The Peace of the Dynasties," the last two focusing upon the fourteenth and fifteenth centuries and constituting the core of the book. Matthew concludes his study with a sketch of Europe around 1500.

The footnoting of this text is arbitrary, sometimes haphazard, and of use largely to specialists. There is no attempt to provide a bibliography, and there are a number of typographical errors. Matthew's book is really an essay about the medieval European community, and, like most essays, it is selective and argumentative. It should appeal to medievalists and non-medievalists alike, however, both for its originality and its guiding theses. Specialists may debate its points, as specialists usually do, but few will fail to profit from Matthew's original views about familiar ground. Nonspecialists could choose far worse introductions to medieval history (they usually do). The heroes of Matthew's essay are the proponents of an ordered, productive, charitable society: craftsmen and connoisseurs, entrepreneurs and inventors, individuals confident in their creative powers and supported by a religious world-view which, despite its faults, affirmed the dignity of nature and humanity and attributed great virtues to productiveness and order. The limited intellectual and material horizons of this world and period did not prevent its members from stepping confidently onto a worldwide stage in the fourteenth and fifteenth centuries, and Matthew's study is one valid approach to the explanation of the rise of European power during the early modern period.

EDWARD PETERS
University of Pennsylvania

SYLVIA L. THRUPP. *Society and History: Essays*. Edited by RAYMOND GREW and NICHOLAS H. STENECK. Ann Arbor: University of Michigan Press. 1977. Pp. x, 363. $18.50.

As scholars begin to review cooperative endeavors between history and the social sciences over the 1950s and 60s there is a painful accumulation of evidence that representatives of these disciplines consistently talked past one another on conference platforms and rarely produced more than convenient administrative superstructures for joint academic programs. At the same time, this postwar generation looms as a heroic age of leaders in the formulation of ideas and communication patterns between these two disciplines. Among these heroic figures, Sylvia L. Thrupp well deserves a prominent place. It is a real service to scholars, therefore, to have her main methodological studies for a period of some thirty years brought together in one attractive and interpretative volume.

When one recalls the acerbic wit, trenchant sallies, and mind-boggling leaps over space and time associated with Thrupp's role as commentator and critic upon so many occasions one realizes that this volume is only a small part of her contributions. I have never heard any of her undergraduate or, indeed, graduate lectures, but I am rather pleased we do not have to depend upon student notebooks! This type of collection is the best that could be done under the circumstances. To assist us further, the editors have organized the twenty-three papers included here under five categories: Social Structure; Social Attitudes; Historical Demography; Social Change; and On Historical Method.

While this organization does serve a certain convenience in isolating some of the main elements of her work, to label any article of Sylvia Thrupp's in this fashion is somewhat forced. For example, of the four articles under Social Structure, surely "Hierarchy, Illusion, and Social Mobility" belongs as much under Social Attitudes. Or, of the four papers under Social Attitudes, surely "Aliens in and around London in the Fifteenth Century" is a solid piece of Historical Demography. In turn, some of the papers classified under Social Change, such as "Tradition and Development: A Choice of Views" and "The Problem of Conservatism in Fifteenth-Century England" deal very much with Social Attitudes.

Introductory essays to each section compensate for these inevitable overlaps. The commentators are eminent international scholars: Philippe Wolff, M. M. Postan, Eric R. Wolf, Thomas C. Cochran. Others as well as myself will immediately see the value of this arrangement for interdisciplinary seminars. Each scholar pays tribute to the *sui generis* qualities of Thrupp's work and usefully attempts to place her various studies within modern historiography and social science methodology. When these scholars attempt to take us farther than the present state of cooperation between the social sciences and history warrant, however, they indicate the great distance that remains to be covered rather than the successes already attained. For example, in terms of methodology the last section of this volume is, of course, by far the most useful. And, fortunately, the introduction to this section by Thomas C. Cochran is by far the most complete.

The bulk of Sylvia Thrupp's work as collected here, and elsewhere, remains substantive history. Many more methodological studies such as those of section five in this volume must be written be-

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fore the historian and social scientist can communicate fully. This task of communication still rests very much with the genius of individuals. If I may hazard a symbolic meaning from the photograph on the back of the jacket, Sylvia Thrupp appears to be in the vineyard (not barnyard as one of her essays suggests) prepared for the harvest. This volume whets our appetite in the expectation that she will indeed harvest more.

J. A. RAFTIS
Pontifical Institute of Mediaeval Studies

GORDON LEFF. *The Dissolution of the Medieval Outlook: An Essay on Intellectual and Spiritual Change in the Fourteenth Century*. New York: New York University Press. 1976. Pp. 154. $7.95.

In this brief, synthetic essay, Gordon Leff distills the results of an eminent career. Continuing his demythologizing of Ockham and nominalism, he also broadens the *retractatio* of his earlier and less nuanced version of late medieval thought while developing a general interpretation of the early fourteenth-century mind. After an initial chapter summarizing his thesis he expands it in three sections treating epistemology, physical science, and spirituality. The topics addressed are massive; the thinkers involved are formidable; the issues, no less than Leff's handling of them, are complex and sophisticated; the material is intractable in the extreme. Yet despite some regrettable oversimplifications and omissions, the most notable being the absence of a chapter on political theory, and despite the failure of his interpretation to mesh perfectly with the evidence in the case of religious thought, Leff succeeds at his Herculean task to an admirable degree.

Leff discards the idea of a medieval synthesis constructed in the thirteenth century and then dismantled by Scotus and Ockham. He substitutes the concept of the intellectual outlook, adapted from T. S. Kuhn's morphology of scientific revolutions and expanded to embrace an era's ruling presuppositions about God, man, and society, as well as the natural universe. From this perspective both later and earlier medieval thought constitute one outlook, bounded by Christianity, classical philosophy, and geocentricity. The transition to the late medieval mentality involved a major reorientation, but it was neither a cataclysmic nor a fundamental change. It gradually modified the secondary characteristics of the medieval outlook but left its basic nature intact.

The perceived discrepancies between thirteenth-century explanations and what they sought to explain which led fourteenth-century thinkers to criticize their predecessors resulted not from the influx of indigestible new empirical data but from a shift in speculative viewpoint. The shift is already visible in the condemnations of 1270 and 1277, which Leff sees more as a catalyst than as a first cause, bespeaking as well as legitimizing the desire to put speculative thought on a different footing. This desire was manifested widely in the fourteenth century, cutting across such stereotypical categories as Franciscan and Dominican, Scotist and Ockhamist, realist and nominalist, *via antiqua* and *via moderna*. The resultant late medieval subdivision of the medieval outlook has two distinctive hallmarks: 1.) topics were fragmented, each being treated in its own terms with no necessary carryover to other topics, even in the thought of one individual; 2.) the empirical was divorced from the conceptual, a polarization linked both to a restriction of the number of demonstrable propositions and to a heightening of the logical criteria of verifiability. Related to this was an overriding sense of contingency as the prime attribute of creation and of omnipotence and freedom as the prime attributes of the Creator. Several paradoxes also resulted: the fourteenth century simultaneously yearned for certitude and expanded the range of merely possible and probable conclusions. It abandoned the central terrain of philosophy and theology while at the same time garrisoning the frontiers with ingenious if peripheral activity.

Trenchant and illuminating insights abound in Leff's book, revealing his magisterial command of the material and his gift for swift and shrewd generation. His perceptions sparkle the most brightly and his thesis is most convincing in the fields of epistemology and physics. His treatment of religious thought is less persuasive. There is more carryover from epistemology to theology than Leff acknowledges. Mysticism, apocalypticism, and the call to reform expressed both empirical experience and a speculative viewpoint perennially available in the Christian tradition. Their contemporary appeal also owes much to the institutional failures of Christian society, and they are less amenable to explanation in terms of the pure *Ideengeschichte* which Leff applies more successfully to other topics. Cavils apart, this is an important book whose subtlety cannot be fully acknowledged in a short review. Leff deserves our warmest thanks for the stimulating contribution his essay makes to the ongoing reassessment of medieval thought.

MARCIA L. COLISH
Oberlin College

DOROTHEE METLITZKI. *The Matter of Araby in Medieval England*. New Haven, Conn.: Yale University Press. 1977. Pp. xi, 320. $15.00.

This study is primarily concerned with Arabic influences on Middle English literature. It falls

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into two parts: "Scientific and Philosophical Learning" and "The Literary Heritage." Dorothee Metlitzki discusses problems of transmission, suggesting that the Crusades were more fruitful in bringing a knowledge of Arabic culture to the West than Runciman acknowledged, but concentrating on centers of learning in Sicily and Spain to which Western scholars had access. She then outlines the history of Arabic studies in England in some detail. A generalized account of Arabic teaching emphasizes the influence of Aristotle as his works were transmitted through Arabic sources and the Arabic preference for a reliance on reason and experience rather than on authority. A final chapter of part one examines Arabic scientific ideas in *The Owl and the Nightingale* and in the works of Chaucer, especially in the Squire's Tale and the Canon Yeoman's Tale.

Part two begins with a discussion of "Arabian Source Books," especially *Disciplina clericalis*, *The Secret of Secrets*, and *The Dicts and Sayings of the Philosophers*. A chapter on "History and Romance" demonstrates that the interest in Armenia reflected in romance shows an actual contemporary concern. Subsequent chapters, however, discuss various kinds of distorted views of "Saracens" in romances, examining such themes as marriage between Christian and Saracen, the Muslim princess enamored of the Christian hero—which appears even in sober chronicles—the character of the converted Saracen, the defeated sultan who denies his idols, and the Saracen giant. Western conceptions of the life of Mohammed are discussed in connection with the general treatment of Islam, which the author finds to be especially tolerant in *Piers Plowman*. Parallels are adduced between the Muslim Paradise and the Land of Cockayne. There is a chapter on Sir John Mandeville and a concluding discussion that includes praise for *Floris and Blanchefleur* as a "genuine reflection of life in an Islamic environment" and an assertion of Arabic influence on the troubadours.

The miscellaneous nature of the materials produces obvious difficulties with organization, and the arguments are sometimes strained. For example, the discussion of astrology in *The Owl and the Nightingale* appears to this reader to neglect the context. Again, Metlitzki adduces the Wife of Bath's "healthy reliance on experience" as an indirect reflection of the doctrine of the teaching of Petrus Alphonsi (p. 19), whereas it is actually an echo of the words of an unsavory character in the *Roman de la rose* whose teachings are hardly salubrious. Nevertheless, the book offers a convenient summary of information assembled with considerable learning concerning a subject not previously treated in a single volume.

D. W. ROBERTSON, JR.
Princeton University

T. H. LLOYD. *The English Wool Trade in the Middle Ages*. New York: Cambridge University Press. 1977. Pp. xi, 351. $27.50.

Although English wool contributed nearly all the country's export earnings for most of the thirteenth century, the wool trade was dominated by aliens, first the Flemings and later the Italians and Germans. Trevor Lloyd, as a result of his study of particular customs accounts, believes that the role played by English merchants in the wool trade in the 1270s and 1280s has been exaggerated. It was not until the 1310s and 1320s, with the establishment of a compulsory staple, that, in his estimation, English merchants did succeed in winning control of the North European market. Once established, however, native merchants continued to monopolize the sale of English wool in Northern Europe and alien trade passed entirely into the hands of Italians who used the sea route to the Mediterranean.

The reasons for the establishment of the wool staple and its frequent switch in location have long been a matter of historical controversy, and here Lloyd pursues an independent line. He stresses the fact that English merchants stood to gain as much from the first establishment of the staple as the crown did and that the arrangement was to their mutual satisfaction. He strongly disagrees with the long-accepted view of George Unwin that the Commons and the lesser merchants, resenting the power wielded by the major exporters, took the initiative in the setting up of the home staple in 1352. Lloyd, on the contrary, suggests that the king initiated the new policy as a result of his concern with the tense relations between England and Flanders and the consequent danger to the goods and persons of English merchants. Furthermore, Lloyd insists that as late as 1362 the Commons as a whole was indifferent to the location of the staple and that the later claim that the Calais staple had been established by parliamentary statute must be dismissed as political propaganda.

In considering the rise and fall in the level of recorded wool exports, Lloyd refutes the hypothesis of R. L. Baker that these fluctuations merely reflect the efficiency of revenue collection. Instead, he is firmly convinced that the available figures do provide a reasonably accurate picture of commercial activity. Wool exports always declined whenever there was war between France and Flanders or civil strife within Flanders. Later, in the fifteenth century, attempts to enforce the bullion statutes further reduced the flow of wool abroad, as alien buyers, under the burden of the new regulations, were unable to purchase as much as before. Our understanding of these trade fluctuations would have been enhanced if a few graphs had been provided in addition to the excellent tables.

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Furthermore, there is no detailed consideration of the wool trade in the context of the general European economy. Thus, the famine of 1315–17 and the Black Death are mentioned only in passing, and Lloyd does not assess their impact on general wage and price levels and consider how this affected the wool trade.

Altogether this is a very important book. Not all historians will agree with all his assessments, but Lloyd challenges so many existing assumptions that no one interested in any aspect of late medieval trade or English history can afford to ignore his work.

MAVIS MATE
University of Oregon

JOHN LE PATOUREL. *The Norman Empire*. Oxford: Clarendon Press. 1976. Pp. vii, 416. $31.50.

The Norman Empire is a work of commanding erudition and exciting originality dealing with "the complex but coherent political structure which the Normans built up in Northern France and Britain during the eleventh and early twelfth centuries." John Le Patourel views this structure neither as an aberrant episode in the history of the English nation nor as a mere prelude to the Angevin Empire. Rather, the Norman Empire was a distinct political entity, much more homogeneous than its Angevin successor. Commencing with the Norman conquest of England, it endured until the Angevin conquest of Normandy ca. 1144 and might have endured far longer had it not been for dynastic rivalries.

Le Patourel begins with a chronological summary of the period, followed by extensive analysis of the government of the Norman kings, the growing unity of their dominions, the forces and methods underlying their expansion, and the appropriateness of the term "Norman Empire" as a description of their polity. In the course of the book, the author develops his theme of Anglo-Norman unity expertly and persuasively. Particularly valuable is his analysis of Norman expansion—the unending need for land with which to reward ever-hungering *fidèles* and the use of the castle-monastery-borough combination as an instrument of conquest and colonization.

Since no two scholars are apt to view a major historical episode in precisely the same way, it is natural that I should find myself unconvinced at certain points in Le Patourel's argument. I am less inclined than he to view the surrounding "vassal principalities" as integral parts of a Norman Empire or, at least during Henry I's reign, as victims of ongoing Norman expansion. These principalities appear to have cost Henry more than they brought in; their role seems primarily defensive—a ring protecting the Anglo-Norman periphery. Le Patourel sees the Norman Empire expanding ceaselessly until Henry I's death in 1135. But the only clear indication that Henry's policy was expansionist is the 1128 marriage of his daughter Maud to Geoffrey of Anjou—"a grandiose plan which would bring all the lands and suzerainties of the counts of Anjou into the Norman complex" (p. 87). There are, however, compelling reasons to doubt that such was Henry's intention. Contemporaries saw the marriage as a defensive diplomatic response to the accession of Henry's rival, William Clito, to the countship of Flanders. In a rare factual slip, Le Patourel places Clito's accession *after* the Angevin marriage (p. 82), thereby obscuring any possible connection between the two events. If, indeed, Henry's relations with the vassal principalities and with Anjou were primarily defensive, then one might view the history of the Norman Empire not as a constant drive to expand but as a two-phased process: expansion followed by creative consolidation.

But these are interpretive issues on which scholars can legitimately differ. Few are likely to respond to a work of such sweeping originality by accepting its every word. Nevertheless, the book will be widely read, deeply influential, deservedly admired.

C. WARREN HOLLISTER
University of California, Santa Barbara

JOHN BELL HENNEMAN. *Royal Taxation in Fourteenth-Century France: The Captivity and Ransom of John II, 1356–1370*. (Memoirs of the American Philosophical Society, number 116.) Philadelphia: The Society. 1976. Pp. xiii, 338. $12.00.

In an earlier volume (1970), John Bell Henneman traced the growth of French taxation from 1322 to 1356. The larger part of this sequel covers the crucial period from the capture of King John II to the breakdown of the treaty of Brétigny in 1369. In a valuable concluding chapter he indicates the lines of fiscal movement from Charles V to Charles VII and shows how the latter built a fiscal absolutism upon the precedents set by the former.

Through careful examination of the year by year fiscal development, Henneman demonstrates the extent to which this period determined the characteristics of key national institutions and processes, or the absence of them. The continuing preference in both north and south to grant taxes through local and regional assemblies made it impossible for the idea of a national assembly to take root. The appearance of brigandage in the late 1350s greatly encouraged that preference: communities,

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in providing constantly against it, understood it most easily as a local threat, even in times of truce, and consequently, whenever war recurred, the several regions of France continued to respond to fiscal needs through local and regional assemblies. Moreover, Charles V used local particularism to his advantage and experienced great success in extracting tax support from such assemblies.

This study demonstrates the importance of Charles V's reign for the future development of taxation in late medieval and early modern France. Through the collection of John II's ransom, the *gabelle* became a lasting and prominent feature of the French tax system. A second prominent tax of the *ancien règime* was the *taille*. Although it did not appear until 1384, Henneman believes it was the direct descendant of the *fouage* or hearth tax which, as a favored fiscal device in the south in the earlier period, was regularized by Charles V and levied as a lump sum collected through direct apportionment. It was also during the 1360s that communities began to broaden their understanding of "common profit" and "evident necessity." Once more, the overwhelming pressure of the roving companies brought them to a new understanding of these concepts. Utterly resistant to any tax except that levied in the face of actual war, they became willing to tax themselves to suppress the lawlessness of *routiers* that had never been defined as war. The collection of the king's ransom, while considered as a feudal aid, also represented taxation in time of truce and contributed to the new understanding.

With the resumption of war in 1369, new war taxes were imposed in addition to local defense levies. The author has discovered, using Montpellier as an example, that by 1370 French communities had come to accept not only peace-time taxes but a tax increase of revolutionary proportions. The tax level had risen by a factor of sixteen over that of 1328 and of five over 1348. While this vastly larger volume of revenue led to military successes in the 1370s, it also set an important precedent in fiscal development.

Confronted with the need to analyze a mass of empirical and statistical material on military actions, assemblies, and fiscal levies, Henneman has succeeded in extracting the significant conclusions from subject matter that does not lend itself easily to elegant presentation.

FRANKLIN J. PEGUES
Ohio State University

ROGELIO PÉREZ-BUSTAMANTE. *El gobierno y la administración de los reinos de la corona de Castilla (1230–1474)*. Volumes 1 and 2. Madrid: Universidad Autónoma de Madrid. 1976. Pp. 442; 310. 1,800 pta.

In the twenty-five years before the death of Ferdinand III and the accession of his son Alfonso the Learned, reconquest Castile expanded in area from a seventh to two-thirds of the Spanish peninsula, making administrative reform imperative. The resulting directive offices and their incumbents (*merinos mayores* and *adelantados mayores*) of the following two centuries are studied here minutely in their judicial, bureaucratic, and military functions, and in terms of the consequences of the reforms both for rationalizing the business of government and for aggrandizing the crown.

Four chapters of roughly fifty pages each survey Spain's previous administrative history, the two offices in law, their internal structure (appointment, control, support, remuneration, accountability, and duration) and their competence (juridical, governmental, and military-diplomatic). Chapter five is the meat of the book, much longer than the other chapters together; it traces the offices in León-Asturias, Galicia, Castile, Andalusia, and Murcia, with appended lists of all recoverable incumbents. A doctoral thesis under the celebrated Luis Suárez Fernández, this work draws upon an impressive array of published studies and sources, but it rests particularly on the 573 documents, from six national or regional archives and thirteen lesser depositories, that comprise volume two. Most of these documents are given only in register-paraphrase, especially the hundred from the thirteenth century; but many, especially from the fifteenth century, are transcribed in full. Well bound and printed, the volumes unaccountably lack an index.

The first half of the text, with its brief subdivisions, is overly schematic and at times superficial. The whole, conceived narrowly as administrative mechanics, prefers a static to an evolutionary approach, and the intimidating detail makes for dry reading. But the book's many merits—completeness, clarity, and logical structure—balance these shortcomings. At every step, moreover, the study is rooted solidly in the documents. These scholarly volumes fill a lacuna, and will be invaluable for the history of medieval law, institutions, administration, and Castilian life.

ROBERT I. BURNS, S. J.
University of California, Los Angeles

DAVID BURR. *The Persecution of Peter Olivi*. (Transactions of the American Philosophical Society. New Series, volume 66, part 5.) Philadelphia: The Society. 1976. Pp. 98. $6.00.

David Burr's monograph is a valuable addition to our knowledge of Peter Olivi and to the disputes in which the Franciscan Order was embroiled during

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the late thirteenth century. Olivi was a controversial and prolific writer who produced over sixty works in his career. Many of these writings caused him to face accusations to which he constructed defenses, only to meet counter-accusations.

To comprehend the debates surrounding Olivi, Burr points out that we must understand the complexity of the man. The author sees Olivi as a true intellectual who tried to bring together the tenets of St. Francis with his own vast perception and knowledge. Thus, to understand Olivi, one must peer into his many-sided mind. Burr devotes his best chapters to this intricacy of thought by analyzing Olivi's positions on theology, philosophy, poverty, and eschatology. In the broadest sense, Olivi recognized throughout his writings a two-pronged attack upon the Christian faith. Burr records him as saying that "while evangelical poverty is being challenged by luxury, Christian doctrine is being threatened by intellectual errors so subtle and enticing that in the days to come even the elect will be sorely tempted."

Intellectually, Olivi stood beside Bonaventure, but he was more outspoken than Bonaventure in his criticism and suspicion of the pagan philosophers then attracting Church scholars. Greek philosophy, Olivi felt, was "born in an alien religious environment"; thus, it was extremely dangerous. Indeed, the infatuation with such ancient writings, particularly Aristotle, had eschatological implications of such magnitude that he saw the introduction of Greek philosophy into Christian theology as the intellectual temptation of the Antichrist.

Although he wrote on many subjects—philosophy, theology, eschatology—it was his advocacy of *usus pauper* for his Franciscan brothers which caused the most stir. As Burr shows, Olivi's rigorous fervor for restriction in use, or daily frugality, as opposed to the acceptable, and more popular, simple renunciation of all property rights is what led him most often to troubles with the leadership of his Order and the Church. Also, whether Olivi liked it or not, his defense of *usus pauper* won the fanatical devotion of the Zealot faction within the Order and further enmity from the Conventuals.

Burr demonstrates good historical criticism at work as he uses unwieldy and frequently unpublished manuscript sources. He strictly adheres to his themes in order to meet his objective of a "general treatment of Olivi's travails." The author's analyses of Olivi's statements and the events surrounding the controversial Franciscan, and his suggestive conclusions, are fresh, straightforward, and should inspire new debate among Franciscan scholars.

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MODERN EUROPE

T. K. SEUNG. *Cultural Thematics: The Formation of the Faustian Ethos*. New Haven, Conn.: Yale University Press. 1976. Pp. xviii, 281. $16.50.

In this ambitious and difficult book T. K. Seung, using a method he calls contextual analysis, traces the shift from the God-centered, allegorical sensibilities of the medieval world to the man-centered, literal outlook of modern times. In literature he finds this shift in the progression from Dante to Petrarch to Boccaccio; in philosophy, from Bonaventure to Aquinas to Duns Scotus. This interdisciplinary study, containing wide-ranging discussions of the Western philosophical tradition, uses philosophical and linguistic analysis to produce fresh readings of texts.

As a model for investigation of cultural changes the book has two major weaknesses. First, since he is discussing a historical process, the author should give more consideration to recent historical scholarship. Historians are not necessarily more acute, but he should at least consider how historians conceive the issues. For instance, he relates Boccaccio's achievement to the "burgeoning bourgeois society of his day" (p. 192), but economic historians since Lopez are very unsure that bourgeois society was actually burgeoning in the fourteenth century.

More important, the underlying conception of the book—that the fourteenth century sees the birth of a modern anthropocentric ethos—is a notion derived from Burckhardt that has been fundamentally restated and often rejected by the last two generations of historians. None of these—Kristeller, Garin, Baron, Trinkhaus, Bouwsma—is in the bibliography, where the most recent historian of humanism cited is Toffanini.

Second, the author has a tendency to allow the logic of his argument to ride roughshod over the intricacy of the details. For instance, contrasting Dante's allegorical sensibility with Petrach's more literal one, he argues against Auerbach's notion that the characters in the *Commedia* are figural. Dante's Virgil, he says, cannot be a historical figure, since the *Commedia* is in the domain of fiction rather than historical record. Yet further on the author says that Petrarch's *Africa* shows his historical sense, since it is "a poetic rendition of a historical incident in strict adherence to historical tradition" (p. 141). In fact, despite the important differences between the historical consciousness of the two men, Petrarch's Scipio is not closer to the historical character than is Dante's Virgil, and neither man would have acknowledged the categorical split between history and fiction that Seung assumes. His general point may be valid, but the issue is more complex than he seems willing to admit.

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Despite these weaknesses, the book is often penetrating and represents an interesting attempt to use interdisciplinary models to cast fresh light on old problems.

DONALD J. WILCOX
University of New Hampshire

EDWARD A. GOSSELIN. *The King's Progress to Jerusalem: Some Interpretations of David during the Reformation Period and Their Patristic and Medieval Background.* (Humana Civilitas: Sources and Studies Relating to the Middle Ages and the Renaissance, number 2. Published under the auspices of the Center for Medieval and Renaissance Studies, University of California, Los Angeles.) Malibu, Calif.: Undena Publications. 1976. Pp. 131. Cloth $16.00, paper $12.00.

According to Edward A. Gosselin, Protestant commentaries on the Psalter removed the "cloak of allegory, tropology, or Christological piety" to reveal the *intentio auctoris* and the genuine historical lessons of the Psalms. In the first part of the book, Gosselin examines the Psalm commentaries of Augustine, Nicholas of Lyra, and Lefèvre d'Etaples (Faber Stapulensis) and concludes that only Nicholas broke ground for the Reformation appreciation of the texts' literal or historical sense. The discussion of the second part of the book, devoted to Luther, Melanchthon, Calvin, but especially Beza, should contribute to a clearer understanding of early Protestant exegesis of the Old Testament. This second section earns Gosselin's volume a place among those special studies whose cumulative effect will be a reconstruction of the Reformation's self- and historical consciousness.

Sixteenth-century reformers were relatively uninterested in extravagant allegories. "Higher meanings" seldom soared above the messages which David's prayers and supplications carry for Protestant trust in God's promise. Allegory "kept within the temporal confines of this world" is allegory that can be counted on to instruct prelates, magistrates, and commonfolk and simultaneously to endorse the "religious autonomy and viability" of King David's "vibrant, personal faith" and of the *Sachlage* of ancient Israel. This, in a nutshell, is the well-documented contention of the second part of Gosselin's book. But, on the basis of his expositions of three pre-Reformation Psalm commentaries, the author implies that the Protestant "recovery" and "rehabilitation" of David was preceded by an extended "Babylonian captivity" during which the Psalms were subjected to violently Christocentric interpretations. Gosselin confessed "some relief" when the sixteenth-century reformers finally "escorted" David into "Jerusalem," a destination made both more heavenly and more wordly by a greater historical consciousness than that which characterized medieval exegesis. Had Gosselin pressed beyond Augustine to Jerome, behind Nicholas to the Victorines, past Lefèvre to Erasmus, however, he would have been more suspicious of his own account of "captivity," "progress," and "rehabilitation." But, as it stands, Gosselin's volume demonstrates that literature of this kind is bound by its own particular conventions (cf. James S. Preus, *From Shadow to Promise*), one of which appears to be an inevitable simplification of medieval typological exegesis. Yet this should not be permitted to detract from the stimulating and suggestive work that Gosselin has done with certain Reformers and the Psalms.

PETER IVER KAUFMAN
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THOMAS MCKEOWN. *The Modern Rise of Population.* New York: Academic Press. 1977. Pp. 168. $14.50.

In this short but extremely important book, Thomas McKeown, a physician and professor of social medicine at the University of Birmingham, offers a tightly reasoned analysis of the extraordinary demographic growth in Europe after 1700. The lines of McKeown's argument are clear: the main cause of mortality was infectious disease, with the level of infection largely determined by the standard of living. The rise in nutritional levels which he, following Langer, attributes to the widespread use of the potato from the mid-eighteenth century on, caused death rates from infectious diseases to fall, especially among young children. The improved sewage systems and water supplies in many areas in the late nineteenth century reinforced the earlier gain. Lowered mortality among children quickly led to a large and sustained rise in population. A secondary cause of death among the young was infanticide, and this, too, waned in the second half of the nineteenth century as the use of contraceptives became more widespread.

The elegance of McKeown's work is in the nature of his proofs. Most of the evidence he adduces is from England after 1838, the first year in which the cause of death was recorded on death certificates. Analyzing the major infectious diseases separately, McKeown demonstrates how in each case the decline in mortality from them preceded, usually by about thirty years, the discovery of any effective chemotherapy. Indeed, the only disease which doctors could prevent before 1900 was smallpox, and smallpox was far less important than tuberculosis, diphtheria, or scarlet fever as a major killer. In no way, therefore, can medicine be credited with furthering European population growth in the eighteenth and nineteenth centuries.

Historical demographers can learn a lot from

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this book, both from its substance and from its methodology. McKeown analyzes post-1838 figures, but shows how they can illuminate the 1700–1838 period as well. He introduces evidence from developing countries today, and discusses the ways in which parallels may and may not be drawn to the "developing" countries of eighteenth- and nineteenth-century Europe. He steers demography away from its occasionally obsessive concern with measurement and into the richer field of explanation.

EVELYN BERNETTE ACKERMAN
Herbert H. Lehman College, City University of New York

This drastic recapitulation scarcely does justice to the subtlety and elegance of the author's argument, or to the clarity and grace of his literary style. The authors on whom he principally draws are Montesquieu, Adam Smith, Hume, Hobbes, and Sir James Steuart, but in developing his argument he ranges back to St. Augustine, Aquinas, and Machiavelli, and cites among many others Bossuet, Calvin, Cervantes, Goethe, Hegel, La Rochefoucauld, Locke, Pascal, Rousseau, Lord Shaftesbury, Spinoza, Vico, and Voltaire. For both style and substance I recommend this book highly.

RONDO CAMERON
Emory University

ALBERT O. HIRSCHMAN. *The Passions and the Interests: Political Arguments for Capitalism before Its Triumph*. Princeton: Princeton University Press. 1977. Pp. x, 153. Cloth $10.00, paper $2.95.

Although written by an economist—a distinguished economist—this small book is actually an essay in intellectual history. As such, it meets the highest standards of the genre.

Briefly stated (and therefore oversimplified), Albert O. Hirschman's thesis is that between the sixteenth and the end of the eighteenth centuries, as a result of both "semantic drift" and intellectual progression, the consensus of moral and political philosophers changed with respect to the value they assigned to man's acquisitive instinct. In his own words, "one set of passions, hitherto known variously as greed, avarice, or love of lucre, could be usefully employed to oppose and bridle such other passions as ambition, lust for power, or sexual lust" (p. 41). And he quotes a passage from Montesquieu which both inspired the title of the book and serves as its epigraph: ". . . it is fortunate for men to be in a situation in which, though their passions may prompt them to be wicked, they have nevertheless an interest in not being so" (p. 73). Hirschman further offers his interpretation of the change in the intellectual climate with respect to moneymaking as an alternative to the Weberian and Marxian explanations for the rise of capitalism. In short, the pursuit of material interests, suitably redefined and applauded by philosophers, tolerated and even encouraged by statesmen and rulers, became respectable. But Hirschman in his concluding chapter ("Reflections on an Episode") does not neglect to point out the ironic consequence of this changed intellectual climate. In the opinion of a subsequent generation of critics the optimistic philosophers of the Enlightenment were wrong: "capitalism was supposed to accomplish exactly what was soon to be denounced as its worst feature" (p. 132).

BERNADETTE BUCHER. *La sauvage aux seins pendants*. (Collection Savoir.) Paris: Hermann. 1977. Pp. xvi, 271. 48 fr.

This fascinating and provocative book has an interest for historians on two different levels, both of which go well beyond what a cursory glance at its contents might imply. Bernadette Bucher's study revolves around the portrayal of Amerindians, and particularly that of the "savage with the hanging breasts" in the engravings which accompany Theodore de Bry's collection of travel literature, the *Grands voyages* (1590–1634). Bucher's interest is in the underlying meaning of the selection of subjects and themes portrayed in the illustrations of the *Grands voyages*. Using the methods developed by Lévi-Strauss for the analysis of oral myths and their structure, Bucher examines the recurrent image of the savage with the hanging breasts as a clue to the structures of thought operating in the minds of Europeans as they examined America and its inhabitants. She asserts that, as compared with the majority of female savages in the *Grands voyages* whose breasts are firm and who indeed have the appearance of classical statues, the savage with the hanging breasts is significant in revealing underlying attitudes toward conquest, colonization, and the Amerindian. Analyzing the settings in which the "deformed" woman appears, Bucher convincingly demonstrates that the artistic rendering of drooping breasts is not simply a more realistic portrayal of Amerindian women. Rather, the sagging savage is "transformed," through the methods of structural anthropology, into a view of America related to European views of the temptation and fall of mankind, and particularly to Protestant attitudes toward damnation, Spanish treatment of the Amerindian, and the increasing attempts to encroach on the Spanish monopoly in America.

Bucher points out, in the course of her analysis, that Protestants interested in colonization (and de

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Bry was one) faced a number of difficulties in dealing with America and the Amerindian. On the one hand, they roundly condemned Spanish cruelty as a justification for their own attempt to replace the Spanish in America. On the other hand, they also found themselves wishing to displace the Indians, and in any case their theological view of man's fall made them far less likely than were Catholics to view the Indians as redeemable. For Bucher, the transformations seen by the anthropologist in the de Bry engravings clearly document these attitudes and unconsciously informed both the choice of themes and the ways in which they were used.

Bucher's work demonstrates the need for historians to examine more carefully the unique elements in the Protestant view of America. By extending the scope of structural anthropology beyond oral myths to iconographic representations within the context of historical events, it also offers exciting possibilities for historians interested in exploring the "mind-set" of another era.

AMY GLASSNER GORDON
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FRITZ WAGNER. *Isaac Newton im Zwielicht zwischen Mythos und Forschung: Studien zur Epoche der Aufklärung*. Freiburg: Verlag Karl Alber. 1976. Pp. 237.

During the eighteenth and early nineteenth centuries the mythological account of Newton obscured the historical figure and the extraordinary scope of his intellectual interests. This study of the historical as well as the mythological Newton rests on two assumptions; the one, methodological, the other, ethical. Methodologically Fritz Wagner's study belongs to the tradition of *Ideengeschichte*, and its historical analysis is heavily laden with metaphysical and even theological terms and concepts. In the best of that tradition the book seeks to explore the interrelation of ideas, religious with scientific, philosophical with social, and the historical consequences of that interaction. In assessing those consequences the author brings to his study the ethical assumption that much of what is wrong with the twentieth century results from the bifurcation of ideas and therefore of man. Science has been divorced from ethical and religious considerations; the spiritual needs of man have been separated from his material concerns and thereby obscured. Much of Wagner's sympathy for Newton arises from the belief that his was a "baroque" mind capable of fusing the divine and the human, the sacred and the profane, the religious and the scientific. That instinct for wholeness Wagner believes was shattered by the French Revolution.

Because the methodological and ethical assumptions, and therefore the historical and philosophical analysis, are almost inseparable in this book it presents certain difficulties for the historian who may not share all of Wagner's assumptions. One can applaud Wagner's concern for the ethical and historical relationship between science and society, but in the end we must ask simply if Wagner has been historically accurate both in his treatment of Newton and of his image in the eighteenth century.

The sections on Newton himself are synthetic and derivative, and the author appears to be interested in making available in German the results of post-World War II Anglo-American scholarship, especially that based upon the vast collection of Newton manuscripts in Cambridge and elsewhere. As such, this discussion of the historical Newton is comprehensive and useful, but not original, and heavily influenced by the author's ethical and philosophical assumptions. On the mythological Newton, however, Wagner overemphasizes the importance of Jean Théophile Desaguliers, scientific experimenter and Freemason, in establishing the myth and in articulating Newtonianism for the eighteenth century. Desaguliers was important, but not as influential as Bentley and Clarke, to name but two of Newton's most widely read associates who shared with Desaguliers a rendering of the Newtonian natural philosophy that was latitudinarian to be sure, but decidedly Christian. Although overestimating Desaguliers' influence Wagner does at least recognize his historical importance as a Newtonian who was also a founder of organized, speculative Freemasonry. But the author makes the mistake of interpreting the official masonry of Desaguliers and Anderson to be deism. If Desaguliers was a deist, purely and simply, what then were Tindal, Collins, Strutt, and the libertines whom he so strongly condemned?

In the analysis offered of the Continental interpreters of Newton, Wagner's treatment is solid, although it does not take account of recent work that has been done on d'Alembert, Voltaire, and others. The single chapter on Bavarian intellectual life in the eighteenth century should be of interest to students of German thought as well as to those scholars interested in assessing the impact of Newtonian science in Catholic Europe.

For historians who have easy access to recent scholarship on Newton and Newtonianism or to the original sources themselves this book, probably not intended for them in the first place, will be of little value. For insight, however, into approaches to the history of science and to the meaning of science in Western culture current among German intellectuals this book could be described as a primary source.

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J. H. LAMBERT. *Cosmological Letters on the Arrangement of the World-Edifice.* Translated with an introduction and notes by STANLEY L. JAKI. New York: Science History Publications. 1976. Pp. 245. $18.00.

J. H. Lambert was a Swiss-German autodidact who made notable contributions to philosophy, mathematics, and the physical sciences. He wrote this work in 1761, however, "to find in the whole world-edifice the kind of order, harmony, variety, transformation, interconnection, perfection, beauty, means, and purpose" (p. 74) which he felt it must have as the creation of an all-powerful, all-wise, and wholly beneficent God. In the Age of Newton he sided with Leibniz, seeking assurance "that in the work of the All-wise no driving wheel gets stuck as often happens with our own clocks" (p. 193). This most perfect of all possible worlds, moreover, was finite in time and space, hierarchically ordered, and everywhere inhabited.

In Lambert's cosmology, the solar system was part of a disc-shaped system of stars rotating about a dark central body, which in turn was part of a larger similar system constituting the Milky Way. There might be, he thought, further levels of hierarchical structure, with the Milky Way system as a component of the next higher order.

Lambert's twenty letters, unfortunately, constitute a long-winded, repetitious, and awkwardly written monologue. In this first complete English translation from the German original, the author's haste seems to have been paralleled by his translator's. The book is marred by far too many typographical errors, not a few gross infelicities of translation, and some errors of fact. Tycho Brahe's system, for instance, is described as having only Mercury and Venus rather than all the planets orbiting the sun as the latter revolves about the earth (p. 240).

The forty-two-page introduction and the notes to the text, however, provide a useful background to the man and his ideas, though a fuller treatment of the context of cosmological and theological thought from Newton to Herschel would have added greatly to the value of the book. One wishes that Lambert at least had given us less matter and more art.

WILBUR APPLEBAUM
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ALBERT SOBOUL. *Problèmes paysans de la révolution (1789–1848): Études d'histoire révolutionnaire.* (Textes à l'Appui: Série Histoire Contemporaine.) Paris: François Maspero. 1976. Pp. 445.

This book is a collection of nineteen articles which Albert Soboul has published, or read at scholarly meetings, over the past thirty years. New introductions have been written for several of the articles and the bibliographical references updated. The book not only covers most of Europe (though the main emphasis is, of course, on French rural life) but also includes one article on the peasantry in nineteenth-century Japan. Though the articles concentrate on the period 1789–1848, Soboul discusses, in the first section of the book, the medieval and early modern origins of Old Regime agriculture; he also pursues limited aspects of the peasant situation up to the early twentieth century. These articles are remarkably unified in theme and approach. There are only two articles whose inclusion I would question: "*Un témoignage: Paul-Louis Courier et la Révolution*" and the last part of "*Une paysannerie asiatique: le cas du Japon.*"

Given that all but three of the articles ("*Mouvements paysans et troubles agraires (1789–milieu du XIX^e siècle),*" "*Mouvements paysans antiféodaux (fin XVIII–XIX^e siècle),*" and "*De la faction des compoix*"), plus an extended introduction, are reprints, one might wonder if such a book is worth publishing. It seems to me that it is because it provides a convenient presentation of the views of the world's leading historian of the French Revolution on the role and problems of the peasantry in eighteenth- and early nineteenth-century European history.

Soboul sees French peasant communities on the eve of the Revolution as having many internal divisions, but maintaining their unity because of bitter opposition to the seigneurial system. It was precisely because of this opposition that the peasantry played an "axial role" in the French Revolution. The peasants, unwilling to accept purchase of seigneurial rights, ignored the legal distinctions in the statutes of August 5 to 11, 1789 which abolished feudalism. For this reason, the compromise between the aristocracy and the bourgeoisie broke down. The freeing of the peasantry was, therefore, forced from below rather than imposed from above as it was, for example, in Prussia. Thus, Soboul says, the peasantry pushed the bourgeoisie further into revolution, e.g., into the complete and unqualified destruction of the old order on July 17, 1793, and thereby brought on the temporary and rather uneasy alliance between the Mountain and the peasants. It was this alliance which ensured that national property would be available in small parcels and permitted the development of that large group of peasants with small- to medium-size land holdings so central to the development of French capitalism in the nineteenth century. Finally, Soboul argues, the Revolution destroyed the peasantry in the course of freeing it. With the end of feudalism the internal cleavages of the peasantry

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increasingly asserted themselves and the traditional community fell easy prey to the development of exploitive capitalism.

Soboul's interpretation of the place of the peasantry in the development of French and European society from the eighteenth to the twentieth centuries has remained consistent over the past thirty years. He is not, however, frozen into his interpretations. In these articles two points on which he has shifted his views are striking. The first is a major one. In all of the earlier articles Soboul insisted, as had Lefebvre before him, that the landless and small peasants resisted partition of village commons and thus were responsible for the slowness with which capitalism developed in the French countryside. Several years ago Alfred Cobban (*Social Interpretation of the French Revolution,* pp. 110–17) suggested that the opposite was true. As a result of the work by A. Ado, Soboul now concurs in the view that landless peasants in fact agitated for partition of the commons. It was the wealthier peasants who resisted and thereby, in Soboul's present view, retarded the development of capitalism. The second shift is minor and indeed involves one phrase in one of the articles. Throughout the 1950s and early 1960s Soboul adamantly asserted the "feudal" nature of eighteenth-century agriculture. Since that time, however, although he still insists on the economic reality of the repression of the prerevolutionary peasantry he is perhaps not so wedded to the term "feudalism." The following statement from a paper presented at the Twelfth International Congress of the Historical Sciences at Vienna in 1965 illustrates this semantical shift and provides a fitting conclusion for Soboul's broad interpretation of the peasantry and the Revolution: "*En faisant table rase de toutes les survivances féodales, en affranchissant les paysans des droits féodaux et des dîmes ecclésiastiques, dans une certaine mesure aussi des contraintes communautaires, en détruisant les monopoles corporatifs et en unifiant le marché national, la Révolution a marqué en France une étape décisive dans la transition de ce qu'il faut bien appeler le féodalisme,* faute d'un autre mot, *au capitalisme*" (p. 348, emphasis mine).

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JOHN R. GILLIS. *The Development of European Society, 1770–1870.* Boston: Houghton Mifflin. 1977. Pp. xvi, 300. $9.95.

This book shows the hand of a teacher who knows and loves his subject and works to make it meaningful to students. Not quite a textbook, it will probably be most useful for classroom purposes, though some insights will stimulate students of the period at any level. The focus is on social-economic developments and political events/structures, particularly those involving attacks on the existing states. Comments on cultural trends, though they include careful background on literacy or the social position of intellectuals, are pointed toward politics—Romanticism as an artistic movement might scarcely have existed—and are brief overall. Diplomatic detail is avoided. Eastern Europe is credited with some activity, though certain generalizations on the impact of international capitalism are questionable, and Italy is allowed to unify; the author is, however, clearly most comfortable with Britain, France, and Germany. In sum, the book shows the strengths and emphases of a social historian willing to come to terms with some, if not all, political and cultural trends, making a bow to Europe while implicitly recognizing that the interesting developments were at this point confined to the Western fringe and that Europe is a misleading historical entity.

The period is well portrayed as a century of transition. John R. Gillis is at his best in showing that industrialization was a century-long process not initially involving many factories. He gives full play to broader demographic and economic trends. Only by 1850–70 does the author see innovation become accepted reality, the revolts stilled, urban life a norm, and nation-state governments entrenched. Attention to the stuff of life—birth, death, childrearing—informs the best sections of the book, and terse statements, as on the complex nature of preindustrial longevity, convey important understanding. One pedagogical device is jarring: brief efforts to show that contemporary conditions in the Third World illumine Europe in transition. Occasionally provocative (for example, intellectual ferment is compared to China's Cultural Revolution as a standard challenge to the use of culture in the age of transition), the approach is usually simplistic and sometimes misleading, as when comparison of demographic expansion involves no assessment of population/resource ratios. Moreover, the book is very teleological: the non-West will make it, by Western criteria of modernization though perhaps through different, even better, means. But there is a teacher's fine sense of urging students to juxtapose current reality, on family size for example, to the past. We are even presented with a preindustrial version of the energy crisis, this one in terms of human energy at the end of the eighteenth century.

The book will not satisfy as a final synthesis. The long sections on social history give way to rather conventional political narrative, often not fully comparable even in periodization; the linkages are not completely made. Social trends 1815–48 are presented too heavily in crisis terms, supplemented by a few outright errors, such as the

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premature claim for a demise of tailoring, and pointed too uniformly toward political revolt.

Of the three main ways to slice a social cake—difficult to pursue simultaneously—social stratification is not consistently examined; there are particular problems in the treatment of the middle class. Gender differences are noted rather clumsily, with heavy reliance on the pedestal image. But the book is dramatically strong on age differentials, the interaction between young and old. Here the author works his own findings on youth into an exciting statement about the dynamics of the period.

We end with a section on "final results," an unhappy term and concept. In fact, in 1870, Western Europe was ready after a century of transition to begin to work out the real implications of industrial society: white-collar groups; new, sometimes subtle forms of protest; leisure; bureaucratization. But Gillis notes most of these incipient developments in a sentence, as if to imply that the essence of the modern world was fully formed a century ago. Shades of Whig history, with (bourgeois) modernization triumphant, though after real travail?

The book remains a sound statement overall, and some flaws derive from the effort to convey concepts briefly rather than from simplistic thinking. One could wish that the publisher had done justice to the attempt; inelegant, unjustified type and lack of footnotes testify to the most recent "final results" of modernization on the presentation of the written word.

PETER N. STEARNS
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MARY S. HARTMAN. *Victorian Murderesses: A True History of Thirteen Respectable French and English Women Accused of Unspeakable Crimes*. New York: Schocken Books. 1977. Pp. x, 318. $15.00.

Mary S. Hartman examines twelve cases of murder committed between 1840 and 1892 in which the accused were respectable middle-class women. Those readers who might suspect the author of historical gimmickry will discover that she is a serious social historian whose aim is not to shock or amuse, but "to uncover the links of experience which bound the notorious women to their more obscure sisters" (p. 268).

Without ignoring individual peculiarities, Hartman portrays the alleged murderesses as real women, not as freaks. An examination of their crimes reveals a pattern to both their behavior and the public's response. Most women, Hartman claims, went through a shifting series of crisis periods involving power relationships, new rights and personal fulfillment, during which a particularly sensitive few resorted to extreme solutions. Neither victims of male domination nor rebels who murdered to challenge outmoded social codes or restricted rights, this few struck out in a desperate private effort to improve their domestic situation.

Stresses connected with marriageability, courtship, and adjustment to marriage characterized the earlier cases, such as those of Marie Lafarge and Euphémie Lacoste whose arranged unions soon turned sour. Although guilty, most of the accused were acquitted, for the early Victorians believed that respectable women were not responsible for their own behavior. The later cases concerned more mature married women, like Florence Maybrick and Claire Reymond, who were trying to reconcile heightened expectations of happiness with the material and emotional disappointments of marriage. Profound social changes had deprived such women of innocence and, the public thought, exposed them to corrupting influences. Thus the courts often denied them the immunity which had protected their predecessors.

Significantly, those on trial were seldom presented as the women they really were. Their secrets were peculiarly female, and the truth-seekers were all men who based judgments more on stereotypical expectations than actualities. Often only the well-bred ladies who defied convention to pack courtrooms and eagerly read every detail of the proceedings actually understood the people and conflicts involved. These spectators and their perceptions of the prisoners in the dock are crucial to the author's claim that the latter were typical in many ways of the anonymous majority. The trials, Hartman asserts, provided the lady onlookers with a diversion, but much more importantly, with a vicarious outlet for personal frustrations. Having experienced difficulties similar to the accused, they could sympathize with their plight, if not their solutions.

Hartman admits that her evidence is sometimes fragmentary and her conclusions speculative. She admits, too, the risks inherent in using the experiences of the exceptional few to determine truths about the typical many. Occasionally the author confuses her readers when comparing cases; and her claim that the activities of the militant English suffragettes corresponded to the increase in crimes of passion by French women around the turn of the century is dubious. However, on the whole her presentation is skillful and convincing.

Victorian Murderesses is a fascinating and valuable study which reveals much about the changing position, attitudes, and problems of ordinary middle-class women in England and France during a difficult transitional period.

KATHLEEN E. MCCRONE
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EDWARD R. TANNENBAUM. *1900: The Generation before the Great War*. Garden City, N.Y.: Anchor Press. 1976. Pp. xii, 463. $10.00.

One speaks of the *fin de siècle* as one speaks of the Church or the Party. Each is readily identifiable, uniquely significant, and infinitely fascinating—not to scholars alone, but to eager day-trippers skimming through the museum of history. Edward R. Tannenbaum has written a guide for the latter, a useful survey of what went on in European society and culture in the quarter century before 1914.

His book, essentially, is about change, and this is reflected in the title of his introduction: "Modernization and its discontents." The rest of the book describes conditions and *mentalités* in cities and countryside; class styles, attitudes, and tensions; the possibilities and impact of economic developments and migrations; religion and other popular entertainments; and how these evolved. It outlines the major political, intellectual, and artistic expressions of the period; provides clear, interesting accounts of the most important figures, ideas, and tendencies; and sets all this in a comparative perspective.

The compendious result hovers at times on the brink of cataloguing, but never lapses into it. The guide is too skillful. Neither dissipated nor stinting, his materials are well ordered, his selections judicious, his comments sensible, his quotations make their point. Prospective students of the *fin de siècle* can embark on his tour with confidence and leave it with profit. They will have found an up-to-date account of the essentials, illustrations that really illustrate the text and, in the backnotes, suggestions for further reading (in English whenever possible). This is a book that I shall recommend to my students.

Of course, there are errors and every specialist may compile his own posy of these. To mention a few, phylloxera did not decimate France's southern vineyards in 1907: the troubles of that year were caused by over-production. Village girls did not refuse to give up ballroom dancing, since tavern and green were still their main stomping grounds. The orphans of Bruno's *Tour de France* ended happily on a farm, yes, but in Orléanais not Gascony. Minor points of fact, and easy to correct. More distressing is the way so much fascinating material is oddly muted in the telling. One may expect that, with so much to say, generalities will drive out nuances. The frequent flatness of the telling is more surprising, especially in view of many excellent anecdotes.

Finally, a more general remark: it is not Tannenbaum's fault that potted accounts of ideology, biography, or cultural ventures are history about as much as anchovy paste is gastronomy. As teachers we have all done the sort of thing he does so well. Perhaps we have not always understood the reasons for the intimate dissatisfaction it sometimes leaves behind. This very successful version of textbook history should give us pause next time we do what we shall go on doing.

EUGEN WEBER
University of California, Los Angeles

RUDOLF VON ALBERTINI. *Europäische Kolonialherrschaft, 1880–1940*. (Beiträge zur Kolonial- und Überseegeschichte, number 14.) Zurich: Atlantis Verlag. 1976. Pp. 528. DM 45.

In the introduction to this study Rudolf von Albertini notes that one must be rather presumptuous to undertake a work of this magnitude. I for one am glad that presumption is still a characteristic possessed by some scholars, enabling them to accept the challenge of a major work of synthesis. In over four hundred densely packed pages Albertini, the Swiss scholar and author of *Decolonization: The Administration and Future of the Colonies, 1919–1960*, presents a history of the colonial experience in India, Ceylon, Southeast Asia, and Africa. While specialists may wince at his treatment of their area in ten pages (the Belgian Congo) or twelve (Burma), all readers will appreciate his command of the monographic and periodical literature, his sensitivity to major interpretive issues, and the abundance of information made readily accessible. Although somewhat more limited in scope, it is superior to Steward Easton, *The Rise and Fall of Western Colonialism* and to D. K. Fieldhouse, *The Colonial Empires from the Eighteenth Century*.

In his treatment of each colonial area Albertini describes the mechanisms of administrative control, examines the colonial economy, notes changes in indigenous social structures, and discusses the development of nationalist movements. His handling of these topics is clear and concise, and an impressive amount of evidence is presented in the text and footnotes. The termination date of 1940 occasionally intercepts fruitful discussion of the nationalist movements, although the author is generally flexible in his observance of the chronological limits. While the book is not structured to maximize its effectiveness as comparative history, Albertini is in sufficient command of his material to make apt comparisons and highlight revealing contrasts throughout the work.

Despite the limitations imposed by the broad scope of this study, Albertini devotes special attention to the question of economic development. Did the economic policies and practices of the coloniz-

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ers foster economic growth and contribute to modernization or did they thwart internally generated processes of economic change, tie Third World areas into disadvantageous, dependency relationships with the capitalist West; and fundamentally contribute to the underdevelopment of the Third World? Albertini is familiar with the literature which argues that colonial economic policies, while expanding the production of raw materials and cash crops and increasing exports significantly, essentially produced underdevelopment in the colonies. But at the risk of sounding like an apologist for colonialism, which he is not, he questions this hypothesis and affirms that the colonial epoch marked a stage in the modernization of the Third World. This conclusion, however, is tempered substantially by his often negative appraisal of the colonial economic systems. This places him somewhere between Lewis Gann and Peter Duignan and Walter Rodney.

Albertini finds the concept of "dual economy" extremely useful in analyzing the economics of empire. In most colonial territories two divergent and distinct economic sectors emerged. One was modern—capital-intensive, market-oriented, and dynamic, tied into the world economic system, and largely in the hands of foreigners. The other was based on subsistence agriculture—relatively static, unprogressive, and supportive of the bulk of the population. The modern sector remained an enclave, generating raw materials and cash crops as was appropriate for a peripheral area in the world economic system. It was unable to provide the impetus for diversified and sustained economic growth, particularly since colonial policies were designed to aid the metropole and did not contribute the state support so necessary for the development of the "late starter." Lopsided development was the result.

The settler colonies of Rhodesia, Kenya, and Algeria are especially striking manifestations of the dual economy syndrome. In these cases almost all government economic assistance was directed to the settler population. The Africans, on the other hand, were viewed primarily as a source of cheap labor. Given this analysis one might question why Albertini rejects the term "exploitation" as an appropriate characterization of the economic relationship between ruler and ruled (p. 402). Yet he does, concluding that the colonial powers did establish some of the prerequisites for economic development. Albertini also recognizes that colonial policies combined with complex indigenous factors to inhibit balanced economic development.

While some might quarrel with aspects of Albertini's work, they will have to contend with his extensive evidence and balanced treatment. One hopes that this work, with its breadth of coverage and excellent bibliography, will become available to a wider audience.

DONALD G. SCHILLING
Denison University

SALLY MARKS. *The Illusion of Peace: International Relations in Europe, 1918–1933.* (The Making of the 20th Century.) New York: St. Martin's Press. 1976. Pp. 184. Cloth $15.95, paper $4.95.

As anyone knows who has tried to teach European diplomacy of the 1920s, it is rather like falling off a cliff. To follow the rich literature on Versailles, there was, until recently, virtually nothing at all. There was no general study of the period, save for two older books, which are interesting but now largely of historiographical interest, E. H. Carr's *International Relations since the Peace Treaties* (1937) and G. M. Gathorne-Hardy's, *A Short History of International Affairs, 1920–1934* (1934) both later carried on to 1939. But things are getting better. Jon Jacobson has written an excellent book on *Locarno Diplomacy: Germany and the West, 1925–1929* (1972), and Charles Maier has published a brilliant, suggestive interpretation of the years from 1920 to 1926, *Recasting Bourgeois Europe* (1975). Now Sally Marks gives us a very good survey, *The Illusion of Peace: International Relations in Europe, 1918–1933*, written with verve and authority.

This book is based on wide-ranging reading in memoirs, in the published diplomatic documentary series and in the British Cabinet Papers and Foreign Office files. It is short—146 pages—and indeed one wishes for more. Despite its brevity, the author's selectivity has kept it from being overladen with detail.

The use of "Peace" in the title and three of the chapter headings may mislead some. This is a book about power, who had it, who did not, and who had illusions about it. For example, she rightly points out that the British guarantee to France against German aggression, given at last in the Locarno Rhine Pact, obviously was dependent on military power for enforcement. As the tiny British army was strung out around the world, "what was left available to Europe was militarily laughable" (p. 72).

The best parts of the book are those on Versailles, on Locarno, and on the turbulent, chaotic, downward years of 1930–33. The author is at her best in a skeptical, often sardonic interpretation of Locarno diplomacy. She focuses on the delicate, shifting testing of each other by Briand and Stresemann, Briand hoping to pin the Germans down to a settlement before they found out how weary of war the French were, and Stresemann, pushing the French and British for concession after con-

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cession, all aimed at liberating German soil and thereby creating the conditions for German rearmament. One of the few places I would quarrel with the author's interpretation is her harsh treatment of Stresemann. "It is often said that a diplomatist must lie for his country and Stresemann was a superlative liar, dispensing total untruths to the Entente, the German people, and his diary with evenhanded aplomb" (p. 64). The conclusions are more traditional: the strident nationalism of the period, the failure to solve the question of Germany's place in Europe, and the weakness of the new states between Germany and Russia, such easy prey when those two powers recovered their clout.

WILLIAM E. SCOTT
Duke University

EDWARD O. SMITH, JR. *Crown and Commonwealth: A Study in the Official Elizabethan Doctrine of the Prince.* (Transactions of the American Philosophical Society. New Series, volume 66, part 8.) Philadelphia: The Society. 1976. Pp. 51. $6.00.

This typically brief contribution to the Transactions of the American Philosophical Society sets out to explain, first, what Elizabethans thought (or were supposed to think) about their queen and her responsibilities and, second, what Elizabeth herself apparently felt her role and obligations to be. It purports to analyze her as the Lord's Anointed, as Defender of the Faith, as prince in relation to Parliament and people and as everyone's "Dread Sovereign Lady." All these manifestations of her sacred majesty are enshrouded in what the English themselves are still fond of calling, vaguely, that "certain something," indefinable but clearly admirable, and here designated the royal mystique.

Like the contemporary homilies the author repeatedly quotes, he does go on and on. There is nothing, of course, wrong with length per se, but when Edward Smith spins out to an inordinate extent a thread of a thesis that is essentially short and simple ("Obedience to princes is a commandment of holy scripture, for therein it is stated that all authority is from God and by Him do princes exercise their jurisdiction," p. 20), the effect, like that of the homilies themselves, is tedious. Overall, the work falls very much between two stools. It ends up reiterating the already obvious ("Subjects must first look to God, the prince of all princes, and then to the queen, their second prince and His deputy on earth," p. 49), after having started out expressing the plainest of ideas—in the kind of jargon that impresses only the semi-literate ("The ecclesiastical structure emerged as a normative one which sought to pervade all Elizabethan society with its salutary values," p. 10). "Too-clever-clever by half," as one of my mentors used to say about similar huffings and puffings that ended up without making the balloon even begin to expand, let alone pop.

MICHAEL B. PULMAN
University of Denver

ROBERT TITLER. *Nicholas Bacon: The Making of a Tudor Statesman.* Athens: Ohio University Press. 1976. Pp. 256. $15.00.

At last we have a Nicholas Bacon as substantial as the sources allow. Rising from Suffolk yeomanry to the Lord Keepership and the Elizabethan Privy Council by ability, hard work, and the exercise of prudence and moderation, Bacon remained firmly Protestant though ultimately a *politique.* Bacon's significance lies in three areas: the politics of East Anglia, the law and the Court of Chancery, and the court and Privy Council. Bacon's rise to prominence in East Anglia was largely responsible for a dynasty of county leaders of strong Protestant views who took over most of the offices associated with the fallen Howards. To the law and the Court of Chancery Bacon introduced an unaccustomed professionalism—he maintained high standards of conduct, avoided corruption, and espoused significant reforms. He made his most enduring mark as a jurist on the Court of Chancery and played a part in the evolution of the equity jurisdiction of that court. In court and Privy Council Bacon maintained a mind independent of both the queen and Cecil. Most comfortable in domestic affairs, Bacon played a role in the recoinage, the Statute of Artificers, and the Northern Rising which entitles him to consideration as an architect of early Elizabethan social and economic policy. The only major aspect of Bacon's career for which there is evidence but with which Robert Titler does not deal is Bacon's role as landowner. The subject has been treated extensively in Alan Simpson's *The Wealth of the Gentry, 1540–1660* (1961), and Simpson's estimates of Bacon's wealth are here modified.

New interpretations of Bacon's career emerge from Titler's study. His temporary disgrace, earlier attributed to his association with Hales' case, is here seen as the result of Leicester's enduring opposition to Bacon. But was it Bacon's unfamiliarity with foreign affairs, diplomacy, and commercial relations and his lack of a broad vision which kept him from reaching the topmost leadership, or might it not have been his lack of social confidence, his failure ever to become a courtier, which held him back and made him a victim of the very class consciousness to which his career is an

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exception? Finally, does Bacon really belong in the first rank of Elizabethan statesmen?

Although we may dispute certain conclusions, Titler's study is the result of detailed research and imaginative interpretation, substantiated by extensive documentation. It is worthy of a place alongside the best of recent Elizabethan biographies.

RICHARD C. BARNETT
Wake Forest University

JOEL J. EPSTEIN. *Francis Bacon: A Political Biography*. Athens: Ohio University Press. 1977. Pp. xv, 187. $12.00.

It is Joel J. Epstein's contention that despite numerous studies of Sir Francis Bacon, his political career has not been sufficiently studied or adequately presented. To this task Epstein's book, *Francis Bacon: A Political Biography*, addresses itself. Slow to achieve political or professional status, Bacon failed ultimately to apply his hard-won power constructively. Here lay the real tragedy of his career (p. 155).

Epstein has written a straightforward account with particular emphasis on Bacon's activities in Parliament. Bacon served over a period of forty years, from his apprenticeship in Elizabeth's reign to his fall in 1621 in the reign of James I. In his early years in the House, Bacon ran afoul of the queen in a speech concerning the subsidy. Thenceforth, he worked consistently to advance himself in royal favor and to promote the interests of the Crown. Epstein stresses his role in the Stuart period as a conciliator who tried unsuccessfully to bring together the king and his Parliaments. A staunch supporter of prerogative, he defended "the Crown against an emerging political dissent" and tried "to neutralize opposition to James" (p. 76). Despite Epstein's careful insistence on the slow growth of "opposition" (pp. 75, 93–94), some of his generalizations suggest an overly simplistic view of Parliament in the Stuart period. Bacon's ultimate fall came, Epstein concludes, because Parliament was "looking to vent hostility against the crown" and found in him "an assailable symbol of royal power" (p. 155). Similarly, Epstein sees debate on impositions as "defiance" of the royal prerogative and of the "practices of legal absolutism" (pp. 86–87).

Some questions, suggested by recent critical scholarship, have not been addressed. What were Bacon's working relationships with others in the House who were attempting to forward the king's and the nation's business? How did Bacon gain his seat in successive Parliaments, particularly in the years before he held public office? (We have only one indication, in a footnote.) Did he feel any continuing responsibility for the locality for which he sat? There is, possibly, a sense of his "country" in Bacon's offending speech on the subsidy in 1593 and in his letter of apology to Burghley (pp. 40–42). Thereafter, was Bacon responsible only to the Crown, free from the tension between loyalties felt by many other MPs? Also lacking is a concrete description of Bacon's political skills, first as MP and later during his brief role as presiding officer of the House of Lords. What tactics did he use in the House of Commons, and as Lord Chancellor did he succeed in directing debate in the upper House?

At certain points, Epstein's presentation lacks perspective and there are errors in interpretation. For example, reference to the attack on Attorney General Hobart's position in the House in 1606 would have thrown light on the similar attack on Bacon in 1614 (p. 104). The bill for the continuation of statutes was more than "the usual substitution for any attempt at far-reaching legal reform" (p. 62). Statements concerning Puritanism and the religious situation in Scotland need correction (pp. 31, 48, 69).

Though it does not present substantial new material, the book provides a convenient summary of Bacon's career. Particularly useful is the interesting review of the literature concerning Bacon in chapters one and ten.

ELIZABETH READ FOSTER
Bryn Mawr College

C. NORTHCOTE PARKINSON. *Gunpowder, Treason and Plot*. New York: St. Martin's Press. 1976. Pp. 139. $8.95.

The discoverer of "Parkinson's Law" attended the same school as Guy Fawkes, St. Peter's in York. His curiosity about Fawkes' connection with the most celebrated "bomb scare" of English history has stimulated him to "explain" to a general audience a familiar story, with an unaccustomed emphasis on the manipulations of Robert Cecil. After briefly describing the hard lot of the Elizabethan Catholic minority and outlining only two earlier conspiracies—Ridolfi's in 1570 "fatal to the duke of Norfolk" and Babington's in 1587 "fatal to Mary Stuart,"—he observes that an Elizabethan plot was usually discovered early by those in power, who then watched "with paternal approval giving it a little discreet help steering it in the right direction" until it was shattered "with the utmost publicity" by a dramatic trial.

James I's traumatic memory of the violence of Bothwell and of the "Gowrie plot" is touched upon before a sketch of Robert Cecil's network of

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vigilant informants is presented. Against this background Parkinson presents a lively narrative of a small clique of minor gentry, resentful at James' failure to honor earlier vague assurances of Catholic toleration, plotting murder and insurrection. Careless leaks of information by one or another of the conspirators are indentified, and astute guesses by Robert Cecil added. The superior intelligence of a king penetrates a famous cryptic warning message, Fawkes is arrested, and the deaths or capture of the rest follow. Later portions of the well-illustrated book are concerned with the ensnarement of the Earl of Northumberland and Henry Garnet, the Jesuit,—"the climax" of Cecil's political career. The final pages are concerned with Cecil's ultimate reward: the order of the Garter.

Parkinson provides no critical bibliography but there is a cluster of footnotes at the end citing Jardine and, occasionally, the recent work of Philip Caraman and Francis Edwards. The book is not a substantial contribution to the controversy surrounding the Gunpowder Plot but an unsubtle tour de force that mixes facts with conjectures. Meanwhile there are distracting comparisons to modern extremists in every chapter. The book has facile generalizations on the complexities of the recusants, is shallow in sampling two Elizabethan plots, and has several inaccuracies on the Continental background. It is simplistic to highlight Robert Cecil and yet leave undiscussed other aspects of the story. Parkinson's curiosity about Fawkes has introduced him to questions he does not begin to answer.

ALBERT J. LOOMIE
Fordham University

D. C. COLEMAN. *The Economy of England, 1450–1750.* New York: Oxford University Press. 1977. Pp. viii, 223. £3.75.

D. C. Coleman's new study of *The Economy of England, 1450–1750* is as much two books as one. In the first of these, for the two centuries before 1650, he retraverses ground which, only recently, he summarized in his useful *Industry in Tudor and Stuart England* (1975). It is scarcely surprising, therefore, that the argument seems here a little tired, confused by the introduction of too many varieties of evidence and held back all too often by "the customary reservation demanded by the presence of learned debate." Before the book is halfway through, however, we come (in chapter 6) to the discussion of the period 1650–1750, where many of the earlier hesitations are cast aside in the stimulating development of a new argument. Briefly, it is Coleman's purpose to isolate those points of growth in the English economy which made this period, in contrast to the experience of other West European nations, "an important century of economic change and opportunity." Among these, of course, was overseas trade, especially the trade with North America. But there were other factors too which prepared the way for rises in the overall standard of living, clearly laying the foundations for England's later competitive advantage. Of these, an increase in agricultural productivity became noticeable from the mid-seventeenth century, accompanied by a regional shift of economic activity northward into areas experiencing advanced urbanization for the first time and by industrial innovations "antithetical to the pre-industrialized economy" and which "by definition must herald its close" (p. 156). In the heading of Coleman's final chapter is the challenge "The Divergence of England," and it is precisely this case for a distinctive development of the English economy after 1650 that he argues here with entirely persuasive authority.

Inevitably, for so short yet so full a study, this book is not easy to read. The insights are many, however, even in the earlier and more confusing chapters, and the approach is both learned and humane. Coleman is no one-track economist, and he many times reminds us of the importance of purely social considerations in the economic development of the state: of status, lineage, and family through his first period, of the expectation of leisure against a new consciousness of clock-time in his second. Quantification, he warns us, is liable to founder on the rock of unreliable statistics. It is one of the best recommendations of Coleman's own work in this "sea of early modern economic history" that we barely notice that the statistics are missing.

COLIN PLATT
University of Southampton

B. A. HOLDERNESS. *Pre-industrial England: Economy and Society, 1500–1750.* Totowa, N.J.: Rowman and Littlefield. 1976. Pp. ix, 244. $12.50.

After a drought when English economic historians produced almost no textbooks on the late pre-industrial (early modern) period, a gentle rain is falling. B. A. Holderness now refreshes us with an exceptionally well-written description of social and economic change from the fifteenth through the eighteenth centuries. His is a textbook in the British rather than the American style, that is, an essay on a range of topics commonly covered in British undergraduate courses, the material synthesized from secondary sources leavened by examples drawn from the author's own research—in this

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case from Holderness' study of the rural economy of that much-investigated county, Lincoln. The book is in the realistic rather than conceptualizing genre of economic history and is thus appropriate for general historians.

Some of the writing rises to the masterly. There are particularly rich commentaries on rural England, as is to be expected given the author's special subject, and a proper place is given to rural domestic industrialization, the neglect of which is an Archilles' heel of those writers whose surveys tend to start where this one finishes, in 1750. There are good discussions of the distribution and diversification of "trades" and of the pre-coal northward shift in the center of gravity of industry, though Holderness rather stresses continuity. He does, however, conclude that "we should sell the period before 1750 short if we did not recognize how much of the essential foundations, of consumer demand and social attitudes, of agricultural surpluses and political stability, had been laid" between the Restoration and then. There are excellent pages on society before the Civil War, the guilds, the patterns of trade, and the "soft" variables like religion and literacy that were related to business enterprise. Of course, any specialist can find points to quibble with, partly perhaps because of the exiguous footnoting, but nothing of which he would seriously wish to disabuse his own students, while economists should discover in the fine-grained patterns here traced out a useful chance to devise explanations of preindustrial growth.

No ready-made model is offered, instead its elements are listed in the conclusion. Holderness sees the principal features of the period as an absence of major negative shocks, considerable division of labor, a relatively small and declining employment share in agriculture, substantial rural industrialization, undamaging governmental actions, little disguised unemployment despite a swelling tertiary sector, and a suitable resource endowment. This is a mixed bag and no attempt is made (admittedly it would probably be forlorn) at quantification, but the work is thoughtfully presented and it challenges thought about the springs of the period's growth.

E. L. JONES
La Trobe University

PETER CLARK and PAUL SLACK. *English Towns in Transition, 1500–1700*. New York: Oxford University Press. 1976. Pp. 176. Cloth $7.50, paper $3.95.

English urban history has been very unequally served by scholars. The origins and early development of the borough were the subject of serious study in the heyday of English medieval constitutional studies, historians of the industrial revolution have given corresponding attention to borough development in the last two centuries. But the awkward interval between the Middle Ages and the industrial era, that of the "preindustrial" town—roughly the centuries between 1500 and 1750—got short shrift until the last generation. But since the Second World War there has been a steady flow of monographs, primarily on individual towns.

This book is effectively a progress report on a generation of scholarship. Necessarily it is incomplete; much work remains to be done on many towns, above all, on the greatest of them—London. And there needs to be more thought about general theoretical structures within which individual town histories can be analyzed. This book is a succinct report of what has been accomplished and a thoughtful consideration of these larger theoretical issues.

The task is not an easy one, since the changes of these centuries are not ones of well-defined direction, and certainly not of sudden or radical alteration. Characteristically, of the six towns over 7,000 (outside London) in 1520, five remain in the list of seven towns over 10,000 in 1700. Nevertheless, their character and importance had shifted in various ways in the intervening centuries. Peter Clark and Paul Slack make a useful distinction between country towns and provincial capitals, suggesting that the former shrank in relative importance while the latter, after a period of decay in the sixteenth century, gained relative advantage over their smaller rivals although at the same time losing it to London. What were true provincial capitals at the beginning of the Tudor age had become mere provincial copies of London under the Hanoverians. The dominance of the metropolis and the related pre-eminence of county society, with tastes and aspirations modeled by court and capital, had gone far to destroy the real autonomy—both cultural and political—which the bigger provincial centers had enjoyed at the end of the Middle Ages.

But this is only one development in a complex of changes which are very fully surveyed in this book. Much emphasis is put on the heavy strains which sixteenth-century developments imposed on the inherited medieval structures. Population increase, a flow of immigration from the countryside, coupled with loss of traditional markets and a movement of industry to rural areas placed a heavy burden on the towns. Yet most of them managed to readjust—and that largely by their own efforts, unassisted by central government—so that by the late seventeenth century a new equilibrium had been reached.

This is a very densely written book, packing an astonishing amount of information and of ideas

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into its compact format. For the specialist—or at least those familiar with the general subject—it is useful and stimulating. For the student it may be a bit more difficult to handle unless he or she has the wide context of monographic reading which the book more or less presumes. But is is an important step forward in focusing a range of disparate research and in pointing out the problems which need to be dealt with if a synthetic view of English urban development in this age is to be attained. One looks forward to a larger, less elliptical, more richly developed study, but it would be well to wait until more returns are in before that is attempted.

WALLACE T. MACCAFFREY
Harvard University

HENRY JOHN STEFFENS. *The Development of Newtonian Optics in England*. New York: Science History Publications. 1977. Pp. viii, 190. $12.00.

Optics, unlike electricity for which there has been a large number of histories since the mid-eighteenth century, has never been a popular subject for historical publication. This is curious, as light has, from at least the time of Newton to that of Einstein, been the major test-case for successive generalizations of scientific world-views. The fact is nicely illustrated in Henry Steffens' work which traces the development, during the eighteenth and into the early years of the nineteenth century, of a Newtonian system of optics out of the elements of Newtonian science.

Steffens' book is a monograph in what must now be called the "classical" tradition of history of science as history of ideas, rather than in the latest mode of social or institutional history. Starting with Newton's optics, placed in the context of seventeenth-century mechanical philosophy as modified by Newton's metaphysical and theological concerns, Steffens traces the creation in Britain during the following century of a "Newtonian" optics which relates to, but differs from, its source. I am not as convinced as Steffens of Newton's agnosticism on certain of the scientific assumptions nor of the extent to which metaphysical and theological issues may be thought to have become irrelevant to science during the eighteenth century. Nonetheless, his first two chapters present a fine study of the changes which take place in a scientific theory as it increasingly departs from the circumstances and constraints of its origins.

The last two chapters describe the introduction by Thomas Young of an alternative to Newtonian optics and the resistance to its adoption. More attention here, as in the first part, to the broader context of ideas might have joined consideration of the conceptual problems of its aetherial medium to the chauvinistic and scientific objections to wave motion. Steffens provides, however, a very good description of Young's non-Newtonian approach to light through analogy with sound and his discovery of interference phenomena.

Although much of the material in this work has been published in scattered places elsewhere, and the broad subject of eighteenth- and early-nineteenth-century optics has been described in two recent general histories (V. Ronchi and I. A. Sabra), Steffens' work is a thoughtful and thoroughly professional synthesis of recent analytical studies of an important science during an important period. His detailed treatment of Robert Smith's *Compleat System of Optics* (1738) is valuable addition. There is one minor historical error and one of science—James II was not "secretly" Roman Catholic when he came to the throne (p. 7) and John Michell's instrument (p. 69) moved freely on its pivot and did not involve torsion. Except for end-notes (an abomination apparently required by economics) and a misplaced line (p. 58) which might cause momentary confusion, the book is well produced. The work would be a happy addition to current literature in the history of science if it had done nothing else except emphasize again "in detail, the fallacy of believing that Newton's science, or anyone else's is objective, free from metaphysical assumptions, and exclusively empirical" (p. vii).

ROBERT E. SCHOFIELD
Case Western Reserve University

PETER EARLE. *The World of Defoe*. London: Weidenfeld and Nicolson. 1976. Pp. xii, 353. £6.95.

Peter Earle has read through all of Defoe's writings, divided the material up according to subject, compared Defoe's understanding of realities in his time with that of modern scholars and Defoe's opinions with those of his contemporaries, and presented an Englishman's view of the world from about 1680 to 1730.

Defoe stood between ancient and modern England, his moral inheritance arguing with his modern sensibilities. He lived by his pen, and his interests reflected the concerns of middle-class popular culture—travel, trade, far-flung adventure, getting ahead, sex and marriage, and the present and future state of England. He was especially interested in the things that later attracted Adam Smith and, like Smith, was disposed to reflect on causes and consequences. Of course Defoe never developed anything resembling Smith's grand scheme of human conduct to save him from inconsistency. On the other hand, although he had a keen ear for terse and telling phrases, he never let words take command. There is a discernible pat-

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tern of prejudices and preferences in his ideas. Earle shows that it is possible to identify the main features of his thought as well as his blind spots and even some of the insincerities of his venal propaganda. One is hard-put to think of anyone who better reflects the mentality of his age than Defoe, and this is therefore a highly useful book.

It is also nicely executed—well written and supported by wide reading in secondary authorities. Naturally in a book that ranges over so many fields specialists will find flaws, and some chapters are stronger than others. The one on Defoe's economics is certainly the strongest. The author's achievement is commendable. The publisher, however, deserves censure for his indifference to the problem of footnotes. They are extremely hard to locate (only a chapter number is given), yet serious readers will find them indispensable, not only for the interesting subsidiary questions they address, but also because without them it is sometimes hard to be sure whether the author is paraphrasing Defoe or presenting some other view.

DANIEL A. BAUGH
Cornell University

P. J. MARSHALL. *East Indian Fortunes: The British in Bengal in the Eighteenth Century*. New York: Oxford University Press. 1976. Pp. 284. $16.25.

British fortunes made in eighteenth-century India, and the perceived promise of others to be made, were objects of compelling fascination for contemporary Englishmen. Historians ever after have been intrigued both by the "Nabobs" themselves and by the seemingly magnetic attraction that drew successive generations of them to the subcontinent.

In this study P. J. Marshall examines a previously neglected dimension of British experience in India, the role of the private merchants in Bengal. Marshall limits his study to the formative years of British activity in Bengal extending from the merger of what had been two East India Companies in 1704, through the conquest and adjustments of the 1750s and 1760s, to Pitt's India Act in 1784. The organization is topical. Carefully reasoned, tightly written, and well-documented chapters focus on the economic setting and the British community in Bengal, the Bengal internal and seaborne trade, and the profits of office for Company servants as they relate to the private trade. A summary chapter entitled "A Settling of Accounts" is followed by Marshall's measured conclusions. The bibliography includes manuscript sources in the India Office Library and other repositories in Britain and Calcutta. Secondary sources are cited in footnotes.

The author characterizes his conclusions as "not only prosaic and unexciting by comparison with stories of pillage and destruction, but. . .also tentative" and subject to displacement by new research. Yet the real merit of the book is its comprehensive examination of the "Nabobs" in action. It details their pursuit of profits and a comfortable retirement in England and analyzes their role in the progression of British involvement in Bengal. Theirs was a quest for wealth and private advantage in the bosom of the East India Company monopoly.

If his conclusions are tentative they are judicious. In analyzing his evidence Marshall provides new insights and suggests alternatives to earlier interpretations while avoiding unwarranted conclusions himself. For examples, he addresses the thesis that the British drained the wealth from Bengal, ruined indigenous entrepreneurs, often appropriating their businesses, and impoverished Bengali artisans and cultivators who worked for them. Weighing the positive and negative effects of British trade and the mid-century conquest and noting the paucity of solid information on the Bengali economy before 1704, he concludes that the British impact was indeed mixed. Yet it is clear that the Bengali mercantile and industrial communities were viable and vigorous before the British arrived, and Marshall demonstrates that the British had to adapt to the local norms at first. Only after Plassey did they begin to shape and develop pre-emptive patterns of their own. It was the Bengali merchants who then had to adapt.

A major addition to British Indian scholarship, *East Indian Fortunes* will remain a standard work on the subject for many years.

EDWARD B. JONES
Furman University

COLIN BONWICK. *English Radicals and the American Revolution*. Chapel Hill: University of North Carolina Press. 1977. Pp. xxii, 362. $15.95.

Within a very short time, Keele University's David Bruce Centre for American Studies has established a leading position in Britain. In the hands of a brilliant young faculty and a creative and energetic director, the systematic study of American life and institutions has gained intellectual stature and scholarly maturity. Colin Bonwick, a member of the Bruce Centre, now adds luster to the Centre and a major contribution to the history of the American Revolution. He has, in fact, provided a new facet to the subject: the impact of the American experience upon radical thought in Britain. The "commonswealth" tradition in American rev-

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olutionary ideology has been analyzed brilliantly by Robbins, Bailyn, and others. Bonwick now takes in hand the corollary: the meaning of the American Revolution for Britain's "Commonwealth" or "Real" Whigs, the intellectual heirs of the same earlier British radicals who provided so much grist for the colonial mill.

Although four types of British radicalism are identified in the early years of George III, two—associated with the names of Bentham and Paine—have little direct relevance, and one—the Wilkesite movement is a mere off-shoot of the "Real" Whigs who provide the central concern of the study. Distinguished from Whig grandees like the Pelhams and Rockingham (who believed they derived a prescriptive right from the Glorious Revolution to govern the kingdom), "Real" Whigs also drew their inspiration from the events of 1688–89, but their numbers came largely from the Dissenters.

Suffering various forms of political and social discrimination, Dissenters were particularly responsive to programs for liberal constitutional reform. The theme is constant in the writings of the Hollises, Capel Lofft, Richard Price, and Joseph Priestley. Supported by liberal or lapsed Anglicans like Granville Sharp, Christopher Wyvill, John Cartwright, and John Jebb, the "Real" Whigs quickly identified the American cause with their own. When John Dickinson wrote that "England must be saved in *America*," they shouted, figuratively, a loud "Amen!"

The belief that America and Britain faced the same peril to liberty, made "Real" Whigs reluctant to accept American independence. As fellow-subjects, the colonials might help work a purification of the Mother Country herself. With the point moot, English radicals cast the United States in another heroic role: as the asylum of liberty, a model for less fortunate lands, Britain, to a certain extent, among them. Although republicanism was, they believed, unacceptable for Britain, the American model ultimately offered a *via media,* a pattern of incremental change free of French Jacobin excesses on the left and Pittite repression on the right.

Bonwick has written so well that it comes as something of a shock to realize that his protagonists—their unending writing and agitating notwithstanding—changed the course of events very little. Isolated from the mainstream of British life and little regarded by even their American friends after 1775, the "Real" Whigs find their places among the patron saints of a future Anglo-American rapprochement. They are worth venerating.

CHARLES R. RITCHESON
University of Southern California

RONALD K. HUCH. *The Radical Lord Radnor: The Public Life of Viscount Folkestone, Third Earl of Radnor (1779–1869).* (Minnesota Monographs in the Humanities, number 10.) Minneapolis: University of Minnesota Press. 1977. Pp. ix, 204. $18.50.

Ronald K. Huch's biography of the most important aristocratic radical of the early nineteenth century is not only well done. It also demonstrates the need for other full-scale biographies of prominent radicals so that we can evolve a comprehensive synthesis of the radical contribution to early nineteenth-century politics. The central thesis of this book is that Radnor's career always reflected his commitment to the Revolution of 1688 and the maintenance of the liberties of the subject which was its greatest legacy. From Blackstone, Radnor learned that it was the responsibility of the aristocracy to preserve those liberties as they had helped to defend them from James II's tyranny. This meant continuously monitoring both the king's actions and those of his ministers.

Radnor, known as Lord Folkestone in his early career in the Commons, was, however, no democrat. He was as concerned with the possibility of the suppression of liberty by revolution from below as by the king's government from above. The aristocracy had a responsibility to stake out a middle and independent ground between these two extremes from which it could institute timely reforms and yet resist unwarranted changes in the constitutional, political, and social fabric. In a sense, therefore, he was a radical by intellectual conviction rather than by temperament.

Radnor's advanced opinions covered the whole range of contemporary issues from economy in government and parliamentary reform to the establishment of free trade. He spoke for the need to modernize English institutions—to make them compatible with contemporary circumstances and to assure their continuation into the future. This was good Whig dogma, but Radnor held more far-reaching views than did the Whigs about the extent of the changes necessary.

Despite his prominence Radnor never held office. Huch demonstrates that the cause lay in his temperament, which was essentially that of an opposition man. Radnor cherished his political independence. He also rightly suspected, given their prosecution for libel in 1831 of his old friend and ally Cobbett, that Whigs, no less than Tories, needed monitoring. Radnor's style also worked against him. He lacked that skill to dissimulate and to compromise so necessary in successful ministers.

This book is well worth the attention of specialists and generalists alike. Huch's awareness of current scholarship enables him to relate Radnor's

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career successfully to the important issues of his time. His biography should serve as an incentive and an example to others working in this area.

CHARLES R. MIDDLETON
University of Colorado, Boulder

MALCOLM I. THOMIS. *Responses to Industrialisation: The British Experience, 1780–1850*. Hamden, Conn.: Archon Books. 1976. Pp. 194. $12.50.

Malcolm I. Thomis, author of a good, critical study of the Luddite movement, has given us an overview of the Industrial Revolution in Great Britain which emphasizes its effects upon the well-being of ordinary men and women. Many books are too lengthy; this one is too short. The sources are for the most part obvious and the analysis (in only 167 pages of text) does not penetrate very far beneath the surface.

The author's intention is to examine the major changes caused by industrialization by discussing the opinions of contemporaries. This approach duplicates not only the work of other historians but also parts of a previous book by Thomis (*The Town Labourer and the Industrial Revolution*). Like the latter book, this recent effort is on the survey course level, though it will perhaps have value to graduate students reading for a qualifying examination. In other words, Thomis offers a shortcut through a difficult topic for inexperienced students rather than a serious work of scholarship addressed to the specialist.

It seems unfair to criticize Thomis for not doing what he did not intend to do, but this book would have been much better if it had included some discussion of the factors which allowed Britain to pioneer in industry. Despite the work of a few individuals such as R. M. Hartwell, this enormously important topic is still the most neglected part of studies on the industrial revolution.

Taking Thomis' book for what it is, superficialities aside, I find his contention that "working-class violence played only a minor part in working-class response" to be accurate. It matches the conclusion that views timely reform as having headed off revolution in the world's first industrial country.

JOHN W. OSBORNE
Rutgers University, New Brunswick

BARRY GORDON. *Political Economy in Parliament, 1819–1823*. New York: Barnes and Noble. 1977. Pp. 246. $21.50.

In his introduction, Barry Gordon marks out territory fascinating to those of us interested in the early political economists. The rest of the book, however, does not fulfill this promise. From the historian's perspective, the work is muddled as to purpose, organization, and direction. It is, moreover, under-researched (only printed sources are used) and overfootnoted.

Gordon writes that he has prepared "a study of the interaction of economic doctrine, political practice, and the train of events during difficult days for the British economy." The book never comes close to achieving this goal. It is instead an account of the views of various members of Parliament on economic questions between 1819 and 1823. These are taken almost exclusively from *Hansard*. The focus is on David Ricardo, and the author shows how such politicians as William Huskisson, H. G. Bennet, Joseph Hume, and Alexander Baring agreed or disagreed with Ricardo's views. Particular emphasis (I think rightly) is placed on the anti-Ricardian ideas of Baring. The questions that concern Gordon—cash payments, Corn Laws, Combination Laws, Bank of England, *et al.*—are certainly important ones, but he simply reports the debates on these issues without showing the least consideration for the realities of politics. In part this explains why Gordon's chapters have an unfinished quality about them. There is seldom an attempt to go beneath the surface.

The book confounds for other reasons. What possible explanation can there be for limiting the story of political economy in Parliament to 1819–23? It might be because those are the dates of Ricardo's tenure in the House of Commons, but surely political economy (however much it is equated with Ricardianism) did not die with Ricardo in September 1823. By cutting off so abruptly, Gordon distorts some of the questions he is analyzing. This is especially true for the Combination Laws. The time limitation also prevents him from adequately developing his contention (probably correct) that political economy served the interests of the great landowners as much as it did the middle class. The best work on the issues raised by Gordon—A. J. B. Hilton's doctoral dissertation "Economic Policies of the Tory Governments, 1815–1830" (Oxford, 1973)—has yet to be published. Hilton's outstanding analysis is complemented by Ellis Wasson's unpublished dissertation "The Young Whigs: Lords Althorp, Milton and Tavistock and the Whig Party, 1809–1830" (Cambridge, 1973).

Finally, it is a curiosity of Gordon's book that most of the interesting material is to be found in the forty-eight pages of annotations. It is difficult to understand why an author would relegate to a distant explanatory footnote information that would brighten and enrich his text.

RONALD K. HUCH
University of Minnesota, Duluth

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R. J. MORRIS. *Cholera 1832: The Social Response to an Epidemic*. New York: Holmes and Meier. 1976. Pp. 228. $16.00.

Cholera was one of the great—if ephemeral—traumas of nineteenth-century Europe; in the early 1830s especially, its impact was so deep and pervasive that a study of social response to cholera is potentially an investigation of every aspect of that society. In recent years social historians have evinced increasing interest in health, disease, and medicine as they have come to concern themselves with every aspect of the life cycle and the life of all men and women, even the inarticulate. Cholera in nineteenth-century Europe provides an ideal sampling device for the historian concerned with such realities—and the ideological responses which they elicited.

R. J. Morris' study of cholera in England in 1832 constitutes an important addition to the new social and economic history. Based on careful research into a variety of printed and unprinted sources—administrative, social, medical, and religious—it provides a lucid account of the epidemic, including illuminating descriptions of administrative response, class differential in attitude and mortality, and—perhaps most revealingly—the clash of lifestyle which emerged as middle-class physicians and administrators sought to impose alien constraints on traditional lower-class social practices (as, for example, in the conflict over the traditional concept of waking as opposed to sanitarians' demands for immediate burial of cholera victims and the avoidance of groups congregating in confined areas). Morris' study will become a standard account of a revealing incident in English history.

In addition to his reconstruction of the sequence of events constituting the epidemic, the author has set himself the more elusive task of analyzing religious and medical ideas (the subject of two of the monograph's seven chapters), and here his results are less satisfying. Morris' treatment of religious ideas and their relationship to social interest is relentlessly functionalist, while his consideration of the medical profession and medical theory adds little to that already known in the literature. (He does, however, suggest an illuminating instance of the way in which a novel idea may be evaluated by contemporaries in terms not of its intrinsic content, but of the status of their original advocates.) The author has chosen, moreover, not to draw systematic comparisons with the response of other cultures to the epidemic. Perhaps he was wise to avoid the difficulties implicit in such comparative analysis, but it does leave an obvious opportunity unexplored. Ultimately, however, the existence of ambitious studies such as these should provide the empirical basis for some future synthesizer to shape a comparative synthesis of world reaction to the cholera pandemics of the nineteenth century—a subject of extraordinary potential interest.

CHARLES E. ROSENBERG
University of Pennsylvania

BRIAN HEENEY. *A Different Kind of Gentleman: Parish Clergy as Professional Men in Early and Mid-Victorian England*. (Studies in British History and Culture, number 5.) Hamden, Conn.: Archon Books, for Conference on British Studies and Wittenberg University. 1976. Pp. xii, 169. $12.50.

The study of the rise of the professions in the nineteenth century is still in its infancy and Brian Heeney's careful analysis of the literature of nineteenth-century English pastoral theology is a significant contribution to this important and underresearched area. Heeney argues that this literature reveals an ideal of the parish clergyman as a "professional man" according to a new nineteenth-century conception of this term. Using the terminology of the sociologist Phillip Elliott, Heeney states that the clergy made the transition in the early nineteenth century from "status professionalism" in which their position as gentlemen was of primary importance in defining their role in society to "occupational professionalism" in which their actual duties in the division of social labor became of relatively greater importance. The clergyman did not cease to be a gentleman, but he became "a different kind of gentleman" with a distinctly religious and other-worldly ideal character, life-style, and set of professional skills.

Heeney suggests that this transformation proceeded with relatively little attention to the theological party controversy which has dominated previous writings on the English Church in the nineteenth century. He states that the writings of these pastoral theologians reveal a broad area of agreement on the professional duties of the parish clergyman, regardless of the author's brand of churchmanship. This argument is, however, somewhat weakened by the large degree of inherent noncontroversiality in pastoral theology itself. Furthermore, in order to make a fully convincing case on this point, Heeney would have had to adopt a broader historical perspective and demonstrate the innovativeness of nineteenth-century pastoral theology through contrast with earlier traditions.

One may also question whether status considerations really declined in importance among nineteenth-century "professional men." The clergy undoubtedly accepted and utilized the ideology of professionalism with enthusiasm, but it seems quite likely that they took up these new ideas to protect their social and financial position. Until the agricultural depression of the late nineteenth century, the clergy benefited fully from the

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wave of agricultural prosperity, but, at the same time, they were increasingly threatened by the hostility of Whig parliaments and the encroachment of other rising professions on their former roles in education, medicine, and local administration. By stressing their important, technical, and onerous duties, the clergy justified their burgeoning incomes.

Although Heeney explicitly excludes the entire area of actual Church practice from his view, the evidence (such as that presented in James Obelkevich's recent *Religion and Rural Society: South Lindsey, 1825–1875*) seems to suggest that the commitment of the clergy to new, expanded duties ended whenever there was interference with their high social position. This was the case even when it was precisely their gentlemanly education and life-style which created an insurmountable barrier to real spiritual influence and sabotaged the effectiveness of their "professional" work. Future students of the professions would do well to ponder this irony.

ARTHUR ENGEL
Virginia Commonwealth University

JOHN BROOKE and JULIA GANDY, editors. *Wellington: Political Correspondence*. Volume 1, *1833–November 1834*. (Prime Ministers' Papers Series.) London: Her Majesty's Stationery Office. 1975. Pp. v, 776. £18.50.

Accessibility to the political thought of Arthur Wellesley, 1st Duke of Wellington, is enhanced by this publication. Although Wellington's career is well known, his political correspondence for 1833–52 has never been published. Ultimately, this series will print political letters to and from Wellington from 1833 through his political retirement in 1846.

The correspondence of 1833–34 reveals political difficulty for Wellington. His disdain for the Reform Bill of 1832 continued, and he depicted it as revolution without direction. Nevertheless, he cautioned conservatives against precipitous action. In August 1834, he wrote the Earl of Aberdeen with guarded optimism for his policy, noting rejections of the Jewish, the University, and the Irish Tithes bills in the House of Lords. On October 15, 1834 he rejected the Duke of Cumberland's suggestion for the formation of a Conservative government. A month later, Wellington reversed his advice and formed a caretaker government! The sparse correspondence with Sir Robert Peel provides faint illumination. This volume concludes with the king's call for consultation; perhaps the next will clarify the Wellington-Peel acceptance of office.

The editors define political correspondence broadly. Purely military and personal letters have been excluded, but those with political import, such as advice to the king on the Mutiny Act and a Conservative editor's solicitation for funds, are included. Wellington's selection as Chancellor of Oxford University in January 1834 is amply reflected. Petitions, pleas, and political exchanges emanate from diverse sources. Foreign affairs provide a consistent topic, but activities within Parliament are not well represented.

The correspondence is printed in chronological order. The editors correct dating errors, comment on signatures, identify lesser-known authors, and add explanatory notations. Foreign language letters, however, are not translated. This and subsequent volumes, assuming consistent quality, must be purchased by all libraries with collections in nineteenth-century British history.

DELBERT F. SCHAFER
Missouri Southern State College

R. J. HALLIDAY. *John Stuart Mill*. New York: Barnes and Noble. 1976. Pp. 151. $12.50.

R. J. Halliday, in this introductory study of the political thought of John Stuart Mill, joins that group of Mill scholars which considers it important to show that Mill's thought, though complex and many-sided, is not riddled with contradictions. Now, although it may be true that sniffers-out of contradiction in Mill are people of ill will like Marx, who accused Mill of trying to reconcile irreconcilables by espousing both laissez-faire and the interests of the working classes, one does no service to Mill by assuming that he is disgraced by the discovery of those paradoxes which are the necessary embarrassments attendant upon uncertainty. This is especially the case if one takes the view, as Halliday does, that "Mill was a cautious and uncertain pessimist."

Halliday approaches the work of Mill through a series of discussions of six areas of continuing concern for him, labelled as follows: "Self-culture, the Eclectic Stance and Toryism," "Romantic and Utilitarian," "Science and the Authority of the Instructed," "*Laissez-faire*, Socialism and Future Society," "Liberty and Individuality," "Government, Bureaucracy and Voting." Such a topical arrangement cannot give us a sense of Mill's chronological development or of the flavor and texture of individual works (except perhaps for *On Liberty* which is discussed in some detail); what it does offer is a sober, honest, and judicious estimate of Mill as an eclectic thinker who could indeed join laissez-faire with socialism, a desire for consensus with a devotion to individuality, rationalism with romanticism. The most original and

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valuable of these thematic analyses is the one which studies the uneasy relation between Mill's respect for the physical sciences (especially mechanics and physics) as a paradigm of unquestioned intellectual authority and his belief in the necessity, for self-development as well as truth, of private judgment.

Two shortcomings of the book are its inattention to Mill's ideas of history, only mentioned as one ground of his eclectic disposition, and its reluctance to explore the implications of Mill's chilling remark that he undertook *On Liberty* because "almost all the projects of social reformers in these days are really *liberticide*."

EDWARD ALEXANDER
University of Washington

M. A. SIMPSON and T. H. LLOYD, editors. *Middle Class Housing in Britain*. Hamden, Conn.: Archon Books. 1977. Pp. 217. $12.50.

This collection of essays examines how the British middle class was provided with housing between 1750 and 1914. Six communities with diverse economic, political, social, and cultural backgrounds are examined. R. Newton's "Exeter, 1770–1870" chronicles middle-class housing requirements, finance, economic climate, and speculative development. His evidence suggests that many houses "were built to meet the specific requirements of clients who helped finance the construction." M. A. Simpson, in "The West End of Glasgow, 1830–1914," provides the most comprehensive study. He describes suburban growth; examines finance, construction, architecture, and rent and sale price of dwellings; and discusses middle-class culture. F. M. L. Thompson's "Hampstead, 1830–1914" succinctly and fascinatingly retraces much of the ground covered in his excellent study *Hampstead: Building a Borough*. Thompson focuses on the politics, economics, and topography of several major estates, examines landowners' objectives, and shows clearly that middle-class lifestyles were more influential than business cycles in regulating construction. T. H. Lloyd's "Royal Leamington Spa" stresses economics and changing life-styles as major forces determining development. K. C. Edwards' "The Park Estate, Nottingham" examines how the Duke of Newcastle's castle park became a middle-class preserve and how it fares today. Finally, J. N. Tarn's "Sheffield" is an architectural study of development in terms of space, design, and topography. Each article would have been strengthened if there had been more maps and more photographs to help the reader more clearly visualize the major details discussed.

Several points emerge from *Middle Class Housing in Britain*: the expanding industrial economy and the emerging middle class created an unprecedented demand for housing and the middle class could afford to buy what it wanted. Also, financing was usually at the local level and construction was not very susceptible to business cycles. This study is not, however, a pioneering work. Such scholars as H. J. Dyos, D. J. Olsen, F. H. W. Sheppard, and F. M. L. Thompson have studied middle-class housing in the context of suburban and estate development in London. *Middle Class Housing in Britain*, however, provides several interesting approaches to studying middle-class housing and suggests that there is a wealth of untapped documents to be searched before a balanced understanding of modern urban life is obtained. In fact, its stated objective is to open "the door to more systematic and comprehensive studies of the history of middle-class housing by students of the several relevant disciplines." It merits serious attention because it has accomplished this admirably.

R. VLADIMIR STEFFEL
Ohio State University, Marion

THOMAS WALTER LAQUEUR. *Religion and Respectability: Sunday Schools and Working Class Culture, 1780–1850*. New Haven, Conn.: Yale University Press. 1976. Pp. xv, 293. $22.50.

Thomas Laqueur began his work, he tells us, as an attempt to confirm E. P. Thompson's hostile interpretation of Sunday schools as an agency through which the middle class tried to impose its own industrially-related values on a nascent working class that ultimately founded its unity on differing cultural norms. Laqueur ended his work by rejecting Thompson's argument. Products of middle-class religious philanthropy after 1780 (though with men of working-class origin among their founders and supporters from the early years), the schools by the nineteenth century had become predominantly working-class in their direction. The curricula reflected the desires of working-class parents, and so the schools must be seen as part of working-class culture inculcating skills and values prized among all classes, rather than imposed by one on the other. The relevance of this argument to a proper interpretation of the argument at the end of Halévy's first volume goes, oddly, unnoticed.

Laqueur offers what must surely be a definitive account of the controversy over the teaching of writing. I find nearly unexceptionable most of what he has to say about what was taught, about the importance of the schools in improving literacy

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(by way of supplement largely), and about the schools as agencies of social control and as diffusers of a kind of popular culture. It is, however, a flawed book, and the flaws, while far from fatal, are instructive.

The data on which statistical argument can be based for this period and in this area are notoriously defective and exasperating: what can be conclusively proved is virtually nil and even what can be effectively illustrated is scarcer than one would like. Laqueur uses statistical argument to good effect to prevent national application of a Lancashire-based argument that the rise of child labor led to the substitution of part-time Sunday schooling for regular day schooling—though why this is relegated to an appendix instead of appearing in chapter four, where the argument pertains, is a mystery. But some of the tables are open to different interpretations. Do the differential percentages of those able to read well among employed pupils in the Birmingham New Meeting school below and above the age of twelve really argue for the effectiveness of Sunday school instruction, or do they indicate the presence in the younger group of numbers of children just learning to read? The improvement may be, in other words, only an age-related truism, hardly needing a complex table to justify it. Again, on page 48, a table suggests a correlation between percentages of sittings in different denominations and percentages of Sunday school enrollments in 1851, though sittings form a remarkably unconvincing basis for comparing anything, given an established Church and the flurry of optimistic or desperate church-building over the preceding generation. Laqueur concedes that his statistical material at best confirms his "literary evidence," and one is left with the regretful fear that the barrage of tables is there because it is fashionable to have them, whether they really help or not.

Laqueur also consistently underestimates sectarian differences. I can see no way gracefully to keep readers aware of the loyalties of people and schools as they are mentioned, but more effort should have been made. What distinguished the tone and teaching of, say, Unitarian and Methodist schools, or one variety of Methodist school against another? Certainly the deprecatory remark that the Unitarian Sunday School Association after twenty-four years of operation had "only" 148 schools takes no notice of the number of chapels in the denomination—it strikes me as an impressive figure. He makes an important point in saying that the collapse of the interdenominational Sunday school movement substituted for a public charity a mosaic of little institutions strengthened by individual commitment, and he recognizes that the interdenominational schools were ridden by tensions from the start. But to say that "interdenominationalism died because of the growth of the Sunday school community and not because of outside political forces" misses the all-important point that the fantastic growth of denominationalism in precisely these years drastically altered priorities across the whole religious spectrum.

The most serious evidential weakness concerns his most important point—the growing working-class domination of the schools. His catalogue of teachers and pupils who gained fame in the working-class movement (pp. 156–57) is compromised by listing John Fielden as a Halifax Chartist and by including John Finch, presumably because of his Owenism, though he had risen from his working-class background to become an iron merchant in Liverpool and a member of the liberal yet aristocratic Renshaw Street Chapel. When teachers and managers are addressed in his contextual portrait of the schools (pp. 91–94), the former get a little over two pages, the latter a mere paragraph. The material to support this probably correct argument effectively can be found in school records, which Laqueur might have sampled more widely. In view of the vast number of sermons, manuals, reports, biographies, pamphlets, and periodicals he has surveyed, that suggestion may seem ungrateful. But the work at the local level remains to be done. It is to Laqueur's credit that he has pointed the way, for those who will follow.

R. K. WEBB
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TRYGVE R. THOLFSEN. *Working Class Radicalism in Mid-Victorian England*. New York: Columbia University Press, 1977. Pp. 332. $20.00.

The expectations aroused by Trygve Tholfsen's influential articles in the *International Review of Social History* in 1961 and in the *Political Science Quarterly* a decade later are admirably realized in this important study of the links between working-class radicalism and middle-class ideology in nineteenth-century English cities. During the years in which he has been exploring the origins of mid-Victorian stability, Tholfsen's own interpretation has undergone modification. Initially subscribing to the traditional view that English workers succumbed to the middle-class cult of respectability, he now regards the situation as more complex and ambiguous: without discarding their radical ideals, Victorian working men increasingly recognized points of affinity with the values espoused by the middle class. The total picture suggests both

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consensus and unresolved discord, "a stable culture in a state of inner tension."

In its more militant formative period working-class radicalism, imbued with egalitarian and democratic values, sought to reconstruct the economic and social order. Yet in the very act of asserting their own dignity, working men were becoming committed to the values which were to underpin the mid-Victorian consensus. Once the middle class ceased to berate the poor for their depravity and began to preach the efficacy of self-help, it became possible to strive for social harmony on the basis of common allegiance to the moral and intellectual improvement of the individual.

But if the ideals overlapped, they were by no means identical. To the working-class radical, respectability implied self-respect and independence, a badge of emancipation from inferiority. In the eyes of the middle class, however, a respectable worker was, above all, respectful, recognizing his subordinate position and the hegemony of his social betters. Respectability was, in Tholfsen's words, "a social category posing as a moral category." Its function was essentially ideological: to enable the middle class to justify its claim to superiority on the grounds of innate virtue. The workers knew that the merchants and manufacturers who endowed mechanics' institutes were the same men who refused to negotiate with trade unions or to sanction the concession of full political rights. Nonetheless, the network of institutions that proliferated in the early Victorian period to promote the interests of working people—unions, cooperatives, temperance organizations, friendly societies—all proved vulnerable to consensus values, especially to the stress on individual improvement. Agitation for reform was conducted, as Tholfsen indicates, in ritual forms that reflected middle-class patterns and the aspirations of the community at large rather than the parochial concerns of one segment of it. Working men and middle-class reformers shared platforms in order to celebrate social progress and moral improvement, elements of cohesion rather than cleavage. In the process the spirit of radical protest was dissipated before it could mount a serious challenge to middle-class ascendancy.

Tholfsen's assessment draws upon an impressive array of sources, and he proves himself remarkably adept at uncovering pertinent illustrative material, whether in a friendly society annual report or in the careers of men like William Felkin and Samuel Smiles. His book is written with the assurance of an historian thoroughly immersed in the literature of his subject. Regrettably, the trenchant analysis is not always matched by felicity of style. The dense, labored prose and repetitiousness make for difficult reading and detract from what is otherwise an illuminating work of scholarship.

F. M. LEVENTHAL
Boston University

GEOFFREY CROSSICK, editor. *The Lower Middle Class in Britain, 1870–1914*. New York: St. Martin's Press. 1977. Pp. 213. $15.95.

The British lower middle class have been little known because they were little noticed. The anti-heroes of the pre-World War I generation, they failed to make much of an impression on their era either by act or rhetoric. This anonymity, along with the disinclination of most British historians to engage in social analysis, has obscured them from view.

Geoffrey Crossick, in a collection of essays from a group of young historians of Britain, has tried to bring the lower middle class out of the shadows. As editor and contributor, he frankly admits that he is merely opening the subject for discussion, and he is prudently tentative in his conclusions on a topic where research has only begun. Yet, in an excellent opening "discussion," he provides a conceptual framework that is likely to shape consideration of the group for some time to come. His colleagues follow with stimulating essays on the roles of religion, the economy, white-collar patriotism, housing, and the differences between industrial and market towns in the lives of the lower middle class.

Part of the problem of discussing the lower middle class has been to differentiate them from the skilled working class whose income-level they shared and the more established middle class whose values and manners they aped. Further, the group included two major divisions: the older petty bourgeoisie of shopkeepers and small businessmen and the new, salaried white-collar workers such as clerks and others. The latter group grew by more than six-fold between 1851 and 1911 and expanded by nearly three-fold as a proportion of the employed male population. The relations between these two groups varied in different parts of the country, as did their standing in the local social hierarchy, yet increasingly the labor elite and the secure middle class perceived them as different.

The sources for examining them are limited but tempt further study. Some references to them occur in contemporary literature and autobiography, such as E. M. Forster's *Howard's End* or Richard Church's *Over the Bridge*. The contemporary press and contemporary social commentators supply important impressionistic evidence that can be buttressed by official records of the census, hous-

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ing, and marriages. Thea Vigne and Alun Howkins make fascinating use of oral reports from small shopkeepers.

From this evidence a number of important themes develop in the essays. The lower middle class, often recruited from the upper reaches of manual workers, were marginal individuals, aspiring to move upward but constantly threatened with downward reversal. The ideal for many was the security of a civil service position. They fully imbibed middle-class values of respectability, often reinforced by religious adherence. They were hard pressed by the economic and social changes of the late nineteenth century that limited their mobility and questioned their value structure. They felt challenged by an increasingly aggressive working class that separated them further from the labor aristocrats and by foreign immigrants and women who threatened to displace them from their jobs. Their frame of reference was likely to be merely local, and they sought security in their homes and families rather than in broader associations.

Compared to the Continent, notably Germany and Belgium, there was little attempt among them to protect their interest by collective or political action. Crossick suggests their class and personal isolation, their devotion to middle-class values of individualism, and their commitment to free trade inhibited them from asserting their collective interest. They lacked the sense of social identity and function of the German *Mittelstand*. Yet Richard Price, in a provocative essay, suggests a parallel between lower-middle-class jingoism and the preindustrial "charivari" that permitted the threatened lower middle class to react against enemies of their jobs and values by demonstrating devotion to patriotic beliefs they attributed to the social superiors they admired. All these essays merit the careful attention of historians who seek to understand the social stresses of late Victorian and Edwardian England.

WILLIAM H. MAEHL, JR.
University of Oklahoma

EUGENE L. RASOR. *Reform in the Royal Navy: A Social History of the Lower Deck, 1850 to 1880*. Hamden, Conn.: Archon Books. 1976. Pp. 210. $12.50.

At one point in this book, when describing courts-martial and flogging, Eugene Rasor comments that there are in the National Maritime Museum at Greenwich a "large number of personally maintained logs and journals, usually in the form of diaries kept by midshipmen." None are included here. It is not that kind of book. The author explains in a footnote that the diaries contain only detailed information on weather and wind and "other extraneous data" (p. 145). The interesting diaries that have come to light have already been edited and published by Henry Baynham in his *Before the Mast* (1971).

Rasor has opted instead to write a more rigorous academic book, relying not at all on personal illustrative sources but rather on reports and returns from the Admiralty papers and the *Digests* in the Public Record Office, on the British Sessional Papers, and on a large and varied selection of books and essays. The academic quality of the book emerges both from its impersonal analytic treatment of these sources and from the form in which it is published. There are 123 pages of text, 40 pages of extended footnotes, and more than 30 pages of a bibliography of books and articles, many of which are only marginal to the subject.

Nevertheless, the book constitutes a useful addition to our knowledge of the British navy in the nineteenth century. Rasor may be too eager to give us the latest bibliography on the large subject of the "revolution in government," or on women's studies and contagious diseases, and the reader may weary at times of "our typical bluejacket," who is never personified; but the book examines in fine detail the problems of recruiting and manning, of discipline and punishment, of mutiny, desertion, and drinking.

These subjects are divided into six chapters, each one broad enough to enable the author to launch into subsections on flogging, sex, venereal disease, and alcoholism—all issues of vital concern in the Victorian passion for moral improvement. The Admiralty was no more immune than the army, or than any branch of government, from the barrage of print and oral rhetoric which insisted on reform. The changes in the British navy from 1850 to 1880 may not have been spectacular, but Rasor was right to concentrate on this brief period after the Crimean War when the foundations were laid for those far-reaching improvements in the life of the ordinary seaman that made possible the navy of Empire and World War in the twentieth century.

ALBERT TUCKER
York University, Toronto

C. C. BAYLEY. *Mercenaries for the Crimea: The German, Swiss, and Italian Legions in British Service, 1854–1856*. Montreal: McGill-Queen's University Press. 1977. Pp. viii, 197. $13.50.

While this book is for specialists on the Crimean War, and more particularly the military aspects of it, the work nonetheless raises some interesting general questions for those historians and social

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scientists interested in the neglected topic of how armies have been raised in wartime. As C. C. Bayley points out, until the Crimean War it was standard practice for the British to recruit on the continent of Europe, particularly in Germany. Even those elements of the army regarded as "English" were dominated by the Highland Scots in the eighteenth century and after 1779, when the long-standing prejudice against recruiting in Ireland was dropped, by Irishmen. In England's far-flung colonial empire, heavy use was made of native troops. The story of how England managed to use the manpower of minorities and foreigners to fight around the world will make a fascinating study, when and if someone writes an overview of it.

Bayley's contribution to the history of English recruiting is important because it focuses on the mid-nineteenth century, when the forces of European nationalism had severely undercut the ability of any country to recruit foreigners for its armed forces. For this reason, and because of the manner in which the recruiting effort was carried out, enlistment of foreign nationals was a failure. In Germany, Switzerland, and Italy, English agents and their contacts abroad tried to raise soldiers for the war in the Crimea with but little success. Governmental opposition abroad meant that recruiting had to be conducted sub rosa in some cases. Further, the English found themselves in competition with the French, who seemed to have been more generous in their offers to prospective troops and more vigorous in hunting out recruits. In addition to difficulties abroad, the Aberdeen Ministry ran into sharp opposition to foreign enlistments at home, in particular from those middle-class constituencies who, as Bayley points out, "regarded foreign enlistments as a practice typical of reactionary governments intent upon finding military tools for the destruction of liberty at home and abroad." As a result of these difficulties, the foreign regiments saw no action in the Crimean War.

Bayley's study is well done, but while there is an introduction which deals with recruiting abroad before the Crimean War, the book would have been better if more attention had been given to placing the recruiting problem in a larger perspective. There is little information, for example, on how troops were recruited at home and where *these* soldiers came from. There is no information on how recruiting was done after the Crimean War, and even the chapter on recruiting before Crimea is sketchy. Still, Bayley has made a start toward improving our knowledge of British recruiting practices, and for that he should be commended.

ARTHUR N. GILBERT
University of Denver

ALAN J. LEE. *The Origins of the Popular Press in England, 1855–1914*. Totowa, N. J.: Rowman and Littlefield. 1976. Pp. 310. $18.50.

During the last decades of the Victorian era and the first of the twentieth century, the newspapers of England underwent a transformation with the rise of a cheap, popular press, often described as the new journalism. Alan Lee examines many facets of this process—technological, educational, financial, and political—and provides a valuable discussion of the often neglected provincial press. A major theme of his study is that, ironically, popular journalism soured the aspirations which Victorian Liberalism had for it. By the 1890s the vision of an educated lower class, eagerly awaiting political instruction from Liberal newspapers, was fading—a disillusionment in part due to exaggerated expectations for the 1870 Education Act. The popular press, reaching a mass audience, had become an integral part of the capitalist system, and in the quest for profits—and satisfied advertisers—it abandoned the tone of political erudition which supposedly characterized the "golden age" of the 1860s and 1870s. "Now a Gresham's law of reason seemed to be working itself out, whereby the cheap and the tawdry, the trivial and the sensational, kept out of the market place the valuable, the authentic, and the serious" (p. 212).

Lee's style is at times stilted, and some passages of his book amount to compilations of figures, names, and titles. The story frequently emerges as history without flesh and blood characters. George Newnes, William T. Stead, T. P. O'Connor, and Alfred Harmsworth are all mentioned, but we learn little about them or their specific contributions to the new journalism. Stead's "Maiden Tribute" campaign, for example, is mentioned once—without explanation. The possibility that it, or the campaign to send General Gordon to the Sudan, might have had an impact on government policy is not considered in the section on press influence. Nor does Lee convey a clear impression of how the popular press looked or sounded. A compulsive footnote-reader might be overwhelmed by the 852 citations, but few of the sources noted are newspapers. Despite such lapses, *The Origins of the Popular Press* makes a valuable and welcome contribution to the literature on the subject.

RAYMOND SCHULTS
Eastern Washington State University

DAVID KYNASTON. *King Labour: The British Working Class, 1850–1914*. Totowa, N.J.: Rowman and Littlefield. 1976. Pp. 184. $12.00.

Since the heyday of G. D. H. Cole, much research and writing has appeared on the pre-World War I

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British world of labor. David Kynaston, who establishes his parameters at 1850 and 1914, sets out to provide a synthesis of that "wealth of recent detailed research" in order to present a coherent and all-encompassing view of those critical years in the development of a British working-class mentality.

Convinced that the role of labor during the course of the nineteenth century was resolutely defensive and materialist and that the workers were unable to grasp theoretical Marxism, Kynaston is determined to correct "the half-truth embodied in the hoary old phrase 'the rise of Labour'." Not all readers will agree, however, with his observation that there was a "relative absence of a consciously working-class interpretation of society" during the pre-1868 period. The argument is much stronger for the years of "Transition: 1868–1880," when Kynaston examines the "deeper-lying intellectual, occupational, residential, religious and cultural influences on the working classes" and generally for the period through 1900.

Yet, the greatest disappointment remains for those who are especially interested in the Edwardian era. The subtitle is deceiving, because this supposed synthesis is not at all comprehensive either chronologically or regionally but is instead a thematic study. For example, although the jacket photo finds Ben Tillett addressing workers during the Great Transport Strike of 1912, anyone specifically interested in the controversial "Labour Unrest, 1910–1914" will have to settle for a cursory ten-page "Postscript" that appears to be tacked on so as "to take the political narrative up to the formative event of the Great War." This reviewer came away from that final chapter with more questions than answers and was doubly disillusioned when the notes and select bibliography were not any more enlightening.

Ultimately, despite these limitations, Kynaston's work is a welcome addition to our knowledge of working-class thinking during the nineteenth century. I do not expect, however, to be alone in wishing that he had stopped in 1900.

RAYMOND G. HEBERT
Thomas More College

MICHAEL BENTLEY. *The Liberal Mind, 1914–1929.* (Cambridge Studies in the History and Theory of Politics.) New York: Cambridge University Press. 1977. Pp. viii, 279. $19.95.

The Liberal decline, most noticeably reflected in the performance of the party at the polls after World War I, continues to fascinate historians. It has reached the status of a "problem" and is attracting a growing number of diverse interpretations.

In this slim, compact, and elegantly written study, Michael Bentley adds a new dimension to the debate. Using the method of his mentor, Maurice Cowling, whose influence is "profound and patent," Bentley explores the mind of Liberals between 1914 and 1929 as revealed in the activities of the Liberal elite—politicians for the most part, but also the "Liberal intelligentsia." Part one examines "The party spirit," and part two, more sketchily, "The wider Liberalism." Within this context Bentley offers a commentary, based on a wealth of primary materials, on the relationship of Liberals to their Liberalism. His account is detailed and presumes a specialist's knowledge of the subject.

Bentley persuasively argues that two divergent outlooks, first formed in response to the creation of a coalition in 1915 and the struggle over conscription, were evident in the party before they were personalized and intensified by the Asquith-Lloyd George split of 1916. The one outlook, associated with Asquith and dominant well into the 1920s, was essentially static, dwelling on the old "faith" and "principles." This concentration on political metaphysics (Bentley labels it a "perverse religiosity") lay at the heart of the Liberal failure after 1914. The old thought-world failed to answer; the static vision militated against realistic evaluations of Liberal options.

The second view, associated with Lloyd George, was secular, flexible, and expedient, but its adherents failed to understand or extinguish the Asquithian element. Even in the 1930s, with most of their former rivals cleared from the field, the Lloyd Georgians failed to set their stamp on Liberalism as a whole. The Asquith Liberals failed to exorcise Lloyd George; the Lloyd Georgians failed to exorcise the Liberal ghosts. The author concludes that "liberalism, to live, required Liberals to die; and sufficient peace for the dead to bury their dead."

No serious student of British Liberalism or postwar politics can ignore this book. But is its thesis on the Liberal decline a "more fundamental truth" than those that emphasize the contextual problems confronting the Liberalism of Asquith and Lloyd George? Many will not credit the mind—or high politics—with a decisive influence over the fate of Liberalism. Perhaps, *pace* Bentley, those contextual difficulties facing the Liberals were insurmountable. If so, was not the preoccupation with an older world, and the strong attachment to old principles ("old men becoming older and old ideas deepening in their familiarity") an inevitable response of a dying party?

J. A. THOMPSON
University of Kentucky

STEPHEN KOSS. *Asquith.* (British Political Biography.) New York: St. Martin's Press. 1976. Pp. x, 310. $12.95.

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Asquith's biographers, most notably Roy Jenkins, have treated him kindly. In recent years, however, historians of modern British politics have criticized his accomplishments to the point of regarding him, as Stephen Koss puts it, "variously. . .as an incorrigible opportunist, a dotard, a pantaloon, and a drunk." This new biography is "an attempt to steady the pendulum," to present a "more balanced view of Asquith's aims and achivements" (p. ix). To achieve this balance Koss integrates both of these points of view and uses each as a corrective of the other. Like Asquith's earlier biographers, Koss sees him as a great prime minister and as a skillful politician who deftly balanced the disparate groups in his party and in his cabinet. In fact, he goes beyond this traditional interpretation to stress Asquith's positive contribution to the legislative accomplishments of the New Liberalism. At the same time, however, Koss accepts a number of criticisms from Asquith's recent detractors. He is particularly critical of Asquith after 1915 when his complacency, his lack of imagination, and his well-known policy of "wait and see" proved his undoing. For Koss, Asquith at the end of his career is not "the last of the Romans," as Jenkins would have it, but a weak man stubbornly clinging to power.

Koss' analysis of Asquith is complex and largely convincing, but in places, I think, he strains too much to prove Asquith's greatness. Asquith was ill suited to handle the violent discontents of labor, women, and the Irish, particularly after 1911. As Koss points out, he was at "a loss to deal with. . .[conflicts] carried beyond the precincts of the Palace of Westminster" (p. 130). A self-created patrician, out of sympathy with democracy, Asquith was at best able to "weather" the crises of the prewar years. Arguably, events had exceeded his capacities several years before the war began.

Using extensive archival material, much of it new, Koss also synthesizes much of the recent monographic work about Liberal politics. For its interpretation of Asquith's career, for its judgments about specific political developments, and for its unusually full discussion of his later years, *Asquith* will be indispensable to students. But it will not completely displace Jenkins' biography, because Koss confines himself entirely to politics. He says little about the interaction of Asquith's public and private lives. He sticks largely to the corridors of power and generally avoids setting Asquith's career in a wider historical framework. Within these self-imposed limitations *Asquith* is a book which no student of modern British politics can ignore.

PETER WEILER
Boston College

DAVID MARQUAND. *Ramsay MacDonald*. London: Jonathan Cape. 1977. Pp. xvi, 903. £12.50.

As a figure in history no less than in the politics of his age, Ramsay MacDonald has suffered a cruel fate. During his lifetime, his reputation perpetually bounced from pillory to post: distrusted and disdained by his remorseless critics on the right, he was too much of a moderate to satisfy his left-wing antagonists, who saw their worst suspicions confirmed by his "great betrayal" in 1931. Since his death in 1937, he has been denounced as a traitor to the cause even by those who had opposed that cause all along. The question has been not so much at what point MacDonald outlived his usefulness, but whether he ever had any.

It has taken MacDonald longer than most of his contemporaries—including Lloyd George, Bonar Law, Baldwin, and Neville Chamberlain—to secure biographical rehabilitation. In a masterful study, David Marquand has performed this spectacular feat. Having made the intelligent decision to project his subject against a sweeping background of domestic developments and international events, he has portrayed MacDonald in a way that is always credible and, at times, surprisingly creditable as well.

The disadvantages, which MacDonald never managed wholly to overcome, were present from the start. After his birth in 1866, his unmarried parents were summoned by the moderator of the local Scottish kirk who sternly lectured them on "the evil of the sin they had committed." MacDonald's father promptly disappeared from his life, but his mother, a redoubtable woman, continued to exert a strong influence long afterwards. The circumstances and aspirations of his youth, illuminated in a semi-autobiographical novel which was mercifully left unpublished, recall various heroes out of H. G. Wells.

A flirtation with the Fabians fortified his socialist predilections. All the same, he remained (perhaps longer than Marquand has indicated) a disciple of the New Liberalism. In 1892, making his first unsuccessful bid to enter Parliament, he bore a Labour label but vigorously insisted that the Liberal and Labour parties should be regarded as "two separate wings. . . [that] form a great Progressive party." It was therefore both logical and advantageous for him to conclude his "non-aggression pact" with the Liberal whip in 1903, paving the way for the return of a Labour phalanx in the general election three years later. MacDonald's constructive efforts to lay the groundwork for the emergence of the parliamentary Labour Party, often forgotten or grudgingly remembered, are skillfully recounted. More clearly than most of his comrades, impatient to assert an identity, MacDonald recognized that cooperation with the Liberals did not preclude independence of them, at any rate until 1924.

A principled pragmatist (Marquand demonstrates that this was no contradiction in terms),

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MacDonald "shied away from doctrinal precision" in politics as in religion. Gradually, he established his authority during the Edwardian period, only to lose it by his stand against British entry into the First World War. Estranged from the movement he had helped to organize, he lacked the solace of his beloved wife, who had died in 1911. Lonely and discredited, he seemed without prospects. Yet, despite his humiliating defeat at the polls in 1918, he quickly rebuilt his career. His opposition to the war, however unpopular at the time, made him "a symbol, even a legend" to a generation that came belatedly to share his moral concern. In 1924, only five years after he had stood forsaken, he took office as the first Labour prime minister.

Marquand examines the policies and predicament of the 1924 Labour government in painstaking detail. Not only for his conduct of foreign affairs but also for his grasp of economic realities, MacDonald receives high marks. Nevertheless, always an intensely introspective personality, he held back from the exultation, continuing to mourn the wife who did not live to share his triumph. "It is an odd thing," he brooded in his diary, "to have a dead man as a Premier to look after a living world."

The "strange death" of Ramsay MacDonald became all the more evident after his return to Downing Street, again at the head of a minority administration, in 1929. Two years later, with the collaspe of international finance, came the crisis in which he exchanged his Labour government for an all-party coalition. To many of those closest to him, MacDonald's behavior seemed "arrogant, disloyal and, in the deepest sense, frivolous." Without ignoring either MacDonald's pitiable vacillations or reprehensible miscalculations, Marquand depicts him as a beleaguered leader who lacked "time to think out what course to follow" and who acted contrary to his own instincts. Thereafter, "the desolation of loneliness" became public as well as private. MacDonald lingered as a figurehead prime minister until 1935, when he made way for Baldwin.

According to Marquand's final computation, MacDonald's "successes easily outweigh his failures." Some, with longer memories or different standards of measurement, will reckon differently. In any case, if this admirable biography does not quite manage to rescue MacDonald from his place among the "pygmies" (C. L. Mowat's epithet) who dominated the interwar years, it succeeds in putting him in better perspective.

STEPHEN KOSS
Columbia University

MELVYN BRAGG. *Speak for England: An Oral History of England, 1900–1975*. New York: Alfred A. Knopf. 1977. Pp. 498. $15.00.

Wigton is a town of 5,000 people located in northwestern England. Melvyn Bragg, with some pertinacity, has drawn several novels and now an oral history from this modest village which, to wax Churchillian, has a lot to be modest about. Presented here is a series of interviews with approximately fifty people whose lives span all or at least half of the twentieth century. The respondents hold no special claim to perpetuity. They are average Englishmen and women who talk about themselves, their work, hobbies, social conditions, wartime experiences, habits, and life styles. The interviews are lightly edited to keep the prose flowing easily, and the reader's progress is not interrupted by the interviewer's questions.

The book is organized into chronological and topical chapters interspersed with chapters devoted to individuals. The section on the interwar period is particularly well done and buttresses the notion that people can cope even in adverse economic conditions. Wigton was no Jarrow and consequently more reflective of daily life in the Slump. The topical chapters, however, come off best. The sections on farming, education, factory work, and religion prove to be the most informative to social and economic historians. Not all is even-handed, for the chapter on religion contains long interviews with a Roman Catholic and a Methodist and then, after an apologetic preface, a Church of England respondent is allotted one page. Perhaps there is a message here regarding the condition of the national church.

The author connects the interviews by weaving background and analysis throughout. One may not always agree with his assessments, but he puts them beautifully. A taste of modern England emerges—how it has changed and how it has remained the same. We are asked, however, to accept the premise that Wigtonians "speak for England." The author offers no verification of this assertion beyond his assurance that it is so. Bragg asks too much here. These stories of individual successes and failures should be accepted only as a slice of life. As history the book has limited value because the information presented is too raw, too diffuse, invariably anecdotal. The lack of a subject index limits its use as a reference tool.

Nevertheless, the author has assembled an engaging, often enlightening book which stands as a solid model for oral history. It compares favorably with Blythe's *Akenfield* in subject matter, although it is more tedious. Too often the book strays from the mainstream and reader interest wanes. The author might well have employed more judicious editing without sacrificing content. Yet, anyone who has interviewed non-elitist respondents must be impressed with Bragg's mastery of technique and sensitivity. He encouraged introspection and persuaded his people to speak candidly. He might be faulted for not including a few interviews from

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those under the age of thirty, for not giving us more political insight, and for not encouraging his respondents to place themselves into a broader setting of English life, but after five hundred pages, dare we ask for more?

STEPHEN R. WARD
University of South Dakota

GRAEME M. HOLMES. *Britain and America: A Comparative Economic History, 1850–1939*. New York: Barnes and Noble. 1976. Pp. 224. $12.50.

Great Britain and the United States have both been front runners in the industrial revolution, leaders in managing world financial markets, and pioneers in devising the twentieth-century welfare state. Their similar histories have stimulated an extensive literature, including work in the "new" economic history, that compares the economic development of the two countries. Graeme M. Holmes has written a thorough synthesis of this literature; his only significant omission is a full comparison of the nations' imperial histories.

The dominant theme is that market forces have explained differences in economic structure, including the character and importance of export markets, and the rates of economic growth. Holmes leans heavily on the notion that each nation's unique set of resources produced different responses to the forces of world markets, and his discussion of the impact of abundant landed resources in America is particularly compelling. He does not assess carefully the role of social or cultural factors that might have affected the relative prices of resources. But he has difficulty in passing over the noneconomic setting of the marketplace. At one point he remarks, without elaboration, that "demand pressures . . . mirrored a host of cultural and geographical circumstances which varied between the two countries" (p. 80). At another, when explaining divergent attitudes toward tariffs, he refers vaguely to the significance of "the psychology of economic beliefs" (p. 100). At yet another, he highlights the stronger tradition of effective central banking in Britain but does not discuss the basis for that tradition.

Holmes has an acute awareness of important questions but he often answers them ambiguously. For example, his explanation for the different rates of recovery from the depths of the Great Depression wanders among the monetarist point of view, Keynesian structuralism, and an emphasis on psychological factors. And, in assessing the impact of tariffs, he faults British policy-makers for adherence to free trade after 1875, yet he suggests that American tariffs after 1895 did little but support monopoly pricing.

Holmes finds the growth performance of the two economies throughout the period 1850–1939 to have been remarkably similar and concludes that "the Anglo-Saxon democracies" have done "none too badly." One wishes that he had told how a consideration of the impact of World War II and "postindustrial" crisis would alter his conclusions.

W. ELLIOT BROWNLEE
University of California, Santa Barbara

LESLIE HANNAH. *The Rise of the Corporate Economy: The British Experience*. Foreword by ALFRED D. CHANDLER, JR. Baltimore: Johns Hopkins University Press. 1976. Pp. xii, 243. $15.00.

Surely it is time to put business history to work for general history, and this is what Leslie Hannah has done. He tells the story of the "corporate economy" in British manufacturing—how it was born in the mergers of the 1920s, how it slowed during the 1930s and 1940s (though beloved then by theorists of "rationalization"), and how it came to maturity in the fresh mergers of the 1950s and 1960s (though attacked then by theorists of "monopoly capital").

The telling draws on Hannah's thesis for the D. Phil. and on a remarkably broad reading of the literature. The book might well be called "new economic history," for it does use economic theory and, less prominently, economic statistics. But in truth Hannah is, as they say in cricket, an all-rounder. He uses a variety of sources in business history, economics, and politics, ranging from the letters of Sir Eric Geddes (he of the axe) to the mathematical theory of concentration ratios.

The theme, soberly and clearly expressed, is that the domination of Britain's economic life by ICI, EMI, GKN, and other alphabetic octopuses began rather suddenly in the 1920s, chiefly through mergers in the automobile and chemical trades of the second industrial revolution. The market concentration to which the mergers led came later than it did in the United States, and its timing—in the midst of unemployment and competition from abroad—assured it of a warmer welcome. After some intelligent rethinking of the literature, Hannah leaves open the issue of whether the sharp fall in mergers during the 1930s and 1940s was caused by the new ease with which monopoly could be attained without merger or by the temporary exhaustion of the gains to economies of scale. In any case, the transformation of manufacturing achieved by 1930, he argues, contributed much to rising productivity. He also believes that the economic and political case for or against the corporate economy remains open.

The shortcomings of the book arise from the very notion of the "corporate economy." It is always a question whether such tags advance or retard historical thinking: the dark ages, the economic take-

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off, and the like. Hannah is well aware that a new monopoly in Britain might well be disciplined by other monopolies abroad or in related industries. He is less aware that his exclusive focus on manufacturing may undercut his story as well. The new leviathan may be a unicorn. Still, such doubts aside, Hannah has made an important contribution to the economic history of recent Britain.

DONALD N. MCCLOSKEY
University of Chicago

MARTIN GILBERT. *Winston S. Churchill*. Volume 5, *The Prophet of Truth, 1922–1939*. Boston: Houghton Mifflin. 1977. Pp. xxvii, 1167. $30.00.

Winston Churchill deserves the expansive biography with papers originally undertaken by Randolph Churchill and continued with considerably greater professional skill by Martin Gilbert. Gilbert has reduced the scope of detail somewhat, although this huge volume still deals with the interwar years in 1,115 pages of text and about four pounds of dead weight. He continues to tell the story—not to draw a portrait because Winston Churchill the man, with one exception, hardly comes alive in these pages—through almost uninterrupted quotations, from Churchill of course, but also from his many correspondents, from Stanley Baldwin and Neville Chamberlain, from almost every key figure in these decades of lost opportunities and mounting disaster.

The years between the wars witnessed two almost diametrically opposed careers for Churchill. In the twenties, once he had again made his peace with the Tory party, he played a central role in addressing the economic and social concerns of the government. Gilbert probably overemphasizes Churchill's contribution to the expansion of social insurance during that period. Neville Chamberlain's role, though not at all disregarded to be sure, was more crucial than appears from the account offered in these pages. Similarly, while Churchill's tenure as Chancellor of the Exchequer is described at considerable length, there is, I think, insufficient attention paid to the impact of economic policies which he accepted dutifully. Despite occasional initiatives as a kind of orthodox devil's advocate—on the return to the gold standard, for example—he accepted all the conventional clichés of the Bank of England mentality that contributed, in part at least, to the collapse of 1931. Had Churchill retired from public life in 1929, he would have been remembered as an imaginative social reformer before 1914, an unsuccessful though perhaps correct advocate of unconventional strategies in the Great War, and as an orthodox, not terribly constructive chancellor in a dull and modestly competent postwar government.

His contributions while out of office in the thirties, however, stand out more and more as the crucial preparation for his dramatic leadership during World War II. Seen coldly in terms of immediate results, his critiques and prophecies were failures. They neither aroused the British people nor convinced many British politicians. Neville Chamberlain, to whose rehabilitation in the field of foreign affairs Gilbert deals a devastating blow, continued to view economic "stability" as more urgent than rapid rearmament. With few exceptions, his Cabinet colleagues subordinated their own judgment, such as it was, to his self-confident and self-deluding predominance.

Perhaps the most fascinating sections of this volume are those which reveal the variety of sources from which Churchill gathered his information about German military preparations and Britain's lack of readiness. Active duty officers, civil servants, a whole coterie of informants, ran substantial personal risks as they violated the Official Secrets Act and other regulations in order to carry out their patriotic duty as they saw it. Churchill was plainly as well informed, as he played the part of Cassandra, as were his governmental critics who took refuge in the superior knowledge they supposedly had from official sources.

One other facet of Churchill's career comes out revealingly in Gilbert's account. He had to work extremely hard at his writing to maintain the standard of living he desired, and he kept at the writing whatever the demands of public life. His dramatic four-volume study of Marlborough and his less effective *History of the English-Speaking People* were very much his creations, however much he used such eminent historians as Maurice Ashley or Keith Feiling or Bill Deakin to help with the gathering and organizing of materials, just as was his enormous production of magazine articles, newspaper columns, collections of essays, and autobiographical vignettes. He needed to write in order to live, but one is constantly amazed at the energy and discipline—despite external appearances—that informed his whole way of life. Such insight into Churchill's work habits is the one exception to the observation that Churchill as a personality is seen but dimly in the pages of this otherwise fine account. It reveals a great deal about some of the intangible assets that were to make a prophet of doom into the architect of British victory in the war that he predicted.

No doubt the multi-volume life of which this volume is a part will stand as the biography of record for Winston Churchill. It is a massive and impressive achievement not likely to have to be redone for a very long time. Nevertheless, despite the huge outpouring of writing about Churchill in recent years, we still need a balanced, one-volume

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biography that will do for him what, say, Robert Blake did for Disraeli or Harold Nicolson for George V. It does not seem too much to wish for.

HENRY R. WINKLER
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HAROLD WILSON. *The Governance of Britain*. New York: Harper and Row. 1976. Pp. xiii, 219. $10.95.

Considering its authorship and the possibilities inherent in its title, one approaches this pleasant, useful, interesting, but not particularly important book with heightened expectations. One hopes—naively, it turns out—that it will deal in a pragmatic and revealing way with the major questions of how Britain is run, not in theory but in practice, and even yield some clues to the connection between the country's continuing difficulties and the kind of government it has. But in fact, the net is not cast so wide. "Governance" is meant in the most limited and exact sense, and we are given a rather detailed account of the role of the prime minister in the British system, along with a view of how the author of the book, Sir Harold Wilson, sees the job. His study has the authority of the man in the know, but it also, indirectly, suggests some of the reasons for Britain's present problems. Informative on the mechanical details of being prime minister, and told sometimes with charm and wit, it emanates a certain smugness, as though the problems, such as they are, need not be thought worrying or in need of analysis.

Sir Harold is ready to argue with his former colleague, Richard Crossman, whose diaries the Wilson administration attempted to keep from publication. Crossman there contended that the prime minister has become excessively powerful. As one would expect, Wilson denies this, but he makes abundantly clear that firm-mindedness and the ability to direct discussion, particularly in Cabinet, are essential to the post. He does agree with Crossman about the vital importance of debate in Parliament, and favourably contrasts the British system to the American. He feels that in Britain the executive is in the center of debate, more vulnerable perhaps, but in a far better position to push through legislation.

On the openness and restrictions that mark the British system he proves more candid than he may have intended, with amusing and instructive comment on the twice-weekly question period in the House of Commons for the prime minister and the ways in which supplementary questions of doubtful relevance are loosely tied to questions originally under discussion. On national security, where the British can be more secretive and high handed than is commonly believed, Wilson argues that it is quite legitimate for the prime minister and the leader of the opposition to discuss these matters privately and that debate in Parliament should be at the vaguest level of generality—limited to disclosing the total sum spent on such endeavours. His chapter on "The Prime Minister and National Security" is only a few lines longer than one page! As he remarks about the answers the prime minister gives to questions thereon in Parliament, they "may be regarded as uniformly uninformative."

There is some sense of historical perspective in the book, although most of the illustrative examples are drawn from Wilson's most recent tenure as prime minister, from March 1974 to April 1976. In certain respects then, the study is complementary to his *The Labour Government 1964–70* (1971). He is particularly interesting on the matter of collective responsibility of the Cabinet, and he offers a striking instance in the "agreement to differ" over the issue of the Common Market. But more often he finds it more convenient to steer clear of issues and puts his emphasis on techniques. He provides a good sense of the prime minister's typical week at home and the heavy demands of international meetings abroad. He has illuminating things to say on the contrasts between the Conservative and Labour Parties—which may be less now with both leaders elected by the parliamentary party. There are still contrasts of style, however. As Wilson remarks, "The Labour Party is always talking about getting rid of its leader and never does; the Conservative Party never talks about ridding itself of its leader and then, in a moment, in the twinkling of an eye, there is a flash of cold steel."

In details and in feeling for the job this is a work of authentic value, and there is a genial tone of authority and personal knowledge that enhances even its conventional wisdom. There are useful hints on how to be an administrator and a keen understanding of the procedures of Parliament. But true questions of power are, on the whole, absent from Sir Harold's study—no doubt, deliberately so.

PETER STANSKY
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KARL MILLER. *Cockburn's Millennium*. Cambridge, Mass.: Harvard University Press. 1976. Pp. xvi, 322. $15.00.

Henry Cockburn is known today primarily as the author of *Memorials of His Time,* that splendid annal of Edinburgh life in the decades preceding the advent of reform. According to Karl Miller, Cockburn's first biographer, this view is justified. Cockburn was above all a man of letters, a historian, indeed, the "Tacitus of Scotland." But, as Miller shows, he was also a leading advocate, a contrib-

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utor to the *Edinburgh Review*, a reformer of Scotland's legal institutions and of its arcane electoral system, and an increasingly ardent patriot. The political millennium which he initially had expected to accompany reform never arrived, and Cockburn became ever more ambivalent about his great achievement. Social and political transformations, he feared, would eventually swamp the "wealth and sense" that Whigs had endeavored to emancipate. Cockburn the patriot also feared that change would destroy Scotland as a distinct social and cultural entity.

This ambivalence only reflects in part the dualism of Cockburn's character. Even before the triumph of reform, the past came to hold a greater appeal for him than did the future. Increasingly the public man required respites among the Pentlands where he assumed a new and private demeanor. There concern for improvement and Cockburn's well-known pugnacity gave way to flights of imagination and peace of mind. Miller argues that dual personality was a common phenomenon in nineteenth-century Scotland. In Cockburn's case he hypothesizes that guilt arising from a repudiation of his father reinforced Cockburn's nostalgia and precipitated his escapes to the "motherly" havens of the Pentlands. Ironically, in the end Cockburn became, at least to an extent, the father he had resisted.

Miller admits that his is not a conventional biography. His wide-ranging speculations are sparked in many instances by autobiographical considerations, and his fascination with the phenomenon of dualism sometimes leads him far afield (chapters 10, 11). Concerned above all to understand the man and his times by analyzing Cockburn's writings, Miller tends on occasion to gloss over details of historical significance. To what extent, for example, was Cockburn responsible for the shortcomings W. Fergusson has discovered in the Scottish Reform Bill? Miller does not convincingly address Fergusson's challenge. He rejects some of Fergusson's criticisms, quite rightly, as being petty, but he implicitly accepts the notion that the bill was badly drafted and filled with loopholes.

Miller sets out to suggest and evoke rather than to define. The speculations, the occasional discursiveness of this well-written book make it all the more interesting and provocative. As well as providing an insight into the career and musings of an important Scottish Whig, Miller offers innumerable insights into the history and character of early nineteenth-century Scotland.

MICHAEL W. MCCAHILL
Brooks School

JOHN R. HUME. *The Industrial Archaeology of Scotland*. Volume 1, *The Lowlands and Borders*. (The Industrial Archaeology of the British Isles.) Toronto: Macmillan of Canada. 1976. Pp. 279. $14.95.

Here is the industrial archeologist's Baedeker for the most heavily industrialized part of Scotland—a county-by-county and town-by-town survey for all those interested in the material remains of industrialization in Glasgow, Lanarkshire and the Clyde, Edinburgh, the Lothians and Stirlingshire, and the Borders. Clearly the compilation of such a gazetteer involved an enormous amount of legwork, map-reading, and indexing, to say nothing of local history research.

I only wish that John Hume had indicated the limits of his definition of industrial archeology. In his preface he goes no farther than saying that in his gazetteer "the emphasis has been on sites where there are complete or substantial remains," but does the accident of survival always make a site worth recording? Did his definition depend on the age (and what sort of age?) or changed use of an industrial site? Apparently it did, for early airfields are excluded.

The compiler's predilection for the external appearances of sites also struck me as rather unfair. Of his seventy photographs only five show the interiors of industrial structures. Also, while tracing manufacturing technology is a far more difficult task than appraising the architectural features of factory shells, machinery is as much a concern of industrial archeology as are edifices. Hume could have made more concessions in listing equipment. It might be argued that machinery is far more transitory than buildings, but is this always the case?

Four Galashiels woollen mills (Gala, Valley, Wheatlands, and Waulkrigg) that are listed in John Butt's *Industrial Archaeology of Scotland* (1967) do not appear in Hume's gazetteer. This omission suggests the need for a note of "also photographed and recorded" sites at the end of each town entry. That such information is being accumulated is confirmed by the book's jacket blurb, which refers to the National Industrial Monuments Survey—about which Hume ought to have said something. Without notes on lost sites already recorded, a gazetteer like this will be rapidly outdated and will need to be complemented by earlier editions or versions. Finally, the bibliography ought to have mentioned Clifford Gulvin's thorough study of the Scottish border tweed industry.

As far as it goes this is an extremely valuable regional survey. I should like to see future volumes in the series extend their horizons as indicated above.

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R. F. FOSTER. *Charles Stewart Parnell: The Man and His Family*. Atlantic Highlands, N.J.: Humanities Press. 1976. Pp. xx, 403. $20.00.

The title of this study might lead a reader to suppose that this is a straightforward biography of Parnell. This, however, is certainly not the case. In his foreword R. F. Foster maintains that this is an "essay in contextual biography," and in his conclusion, that it is an "exercise in contextual history." He does not, however, enter into a discussion anywhere of what is involved in contextual biography or history, though one might be forgiven for thinking that perhaps all history is contextual, and that which is biographical is only somewhat less so in the interest of a sharper focus. In any case, the author also explains in his foreword that he has "avoided discussing Parnell's political life," in the interest of recreating that social context of which his subject was a product.

Briefly summed up, some very assiduous research on four generations of Parnells in the County Wicklow reveals that Parnell's great grandfather, Sir John, was not, as he has been traditionally represented, a true "Patriot," in resisting the Act of Union between Great Britain and Ireland in 1800; that Parnell's grandfather, William, a controversialist with radical propensities, may have had more influence on him than has been previously thought; that his father married unwisely and unhappily, and by dying relatively young left his family in a very precarious financial situation. Parnell's mother, the daughter of the celebrated American, Admiral Charles Stewart, was a strange and perhaps unbalanced creature, while his sisters and brothers can apparently be divided essentially into the political and the apolitical, with one brother, John Howard, being a curious hybrid. The Wicklow gentry, among whom Parnell grew up, moreover, were apparently not typical of their class throughout Ireland. They not only were more numerous on the ground, especially in East Wicklow, but also more traditionally liberal in their politics, and less threatened as a garrison than elsewhere, at least up until the political emergence and ascendancy of their most famous son. Finally, Parnell himself was less interested in farming, about which he was better informed than has been generally assumed, than in his sawmill, his quarries, and his mining ventures, in which he invested and lost a considerable amount of money.

In all of this, however, Parnell remains the shadowy, enigmatic, and ambiguous figure he has always been. Though many of the myths that surrounded him, especially in relation to his family and background, have now been mercifully and finally laid to rest, the basic question still remains—what made the great man go? In effect, this evaluation of Parnell's role as a Wicklow country gentleman does not reveal those qualities that made him the most dynamic and powerful Irish political figure of his generation. Even some of what might be considered the more obvious connections are not made. For example, Parnell's speculative ventures on his Avondale estate indicate that he was a gambler of no mean nerve, and this certainly carried over into his political life. Indeed, there was perhaps no great Victorian radical politician from Disraeli to Lloyd George who did not exhibit this apparently necessary quality or instinct. Still, when all is said and done, the considerable amount of very real work that went into the making of this volume is a testimony to the industry and tenacity of the author, and now that the greatest part of the groundwork has been done, the way has been cleared for whoever will choose to try to tell us what really made Parnell go.

EMMET LARKIN
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DANIEL J. O'NEIL. *Three Perennial Themes of Anticolonialism: The Irish Case*. (Monograph Series in World Affairs. Volume 14, book 1.) Denver: University of Denver, Graduate School of International Studies. 1976. Pp. xix, 131. $3.50.

The three themes taken up by Daniel J. O'Neil are self-reliance, self-sacrifice, and exploitation. The first he describes as the tendency to deride the culture and the institutions of the imperial power and to exalt those of the native people; the second, the disposition to see suffering, heroism, and a readiness to die—to the extent of placing a high value on violence—as the means of achieving independence; and the third as the belief that the imperial power exploited the subject people and crippled their development. In applying these themes to the Sinn Fein Revolution in Ireland O'Neil shows a fully competent knowledge of modern Irish history.

He selects three historical characters as expressing these themes—Arthur Griffith (self-reliance), Patrick Pearse (self-sacrifice), and James Connolly (exploitation). The author sees all three as largely unsuccessful in achieving their aims. Griffith hoped that independence would produce an industrial revolution based on private enterprise. It was only many decades after the coming of independence that a substantial degree of industrialization (but not an industrial revolution) was attained, and then by state intervention rather than by private enterprise. Pearse's dream of a Gaelic-speaking independent Ireland has taken shape in an English-speaking twenty-six county state. Connolly's hope of establishing a semi-

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Marxist socialist workers state scarcely survived his own execution in 1916.

O'Neil sees the three themes, whether in Ireland or any other part of the world, as somewhat unrealistic. The evils attributed to imperial rule were usually exaggerated while the benefits expected from independence were frequently utopian. By implication the author warns us that exultation at the victory of anticolonialism over imperialism in this century must be tempered by the consideration that victory is won at a considerable cost in hatred and violence and frequently leads to only limited benefits. This book is worthy of serious consideration.

MAURICE R. O'CONNELL
Fordham University

FREDERICK J. BAUMGARTNER. *Radical Reactionaries: The Political Thought of the French Catholic League.* (Études de philologie et d'histoire, number 29.) Geneva: Librairie Droz. 1975. Pp. 317.

Between 1585 and 1594 a Holy League committed to keeping France Roman Catholic effectively controlled Paris and much of the kingdom. In their resistance to the possibility of a Protestant monarch, League political theorists developed increasingly radical arguments against royal policy and supported their position with a barrage of books and pamphlets ranging from reasoned treatises on fundamental law to vitriolic polemical broadsides.

Frederick J. Baumgartner studies in detail League political literature of this period. In so doing, he indicates that his primary purpose is "... to examine a considerable body of works which the League added to the growing corpus of resistance theory which has had such an important impact on modern history" (p. 23).

Baumgartner begins by carefully placing the growth of the League in the political and economic context of the time and then sketches the development of its political thought from its founding in 1576 to the Edict of Union in 1588. After the Edict, in which Henry III granted nearly all of the League's demands for a hard line against Calvinism, the movement mainly concerned itself with opposition to the succession of a Protestant prince and began to use propaganda as an important part of its program. The main portion of the book is devoted to a close analysis of a wide variety of League political works. Then follows a chapter on efforts to substitute the Cardinal of Bourbon as heir apparent to the throne in the place of the Protestant Henry of Navarre. Baumgartner next examines the disintegration of the Catholic Right after 1590. His conclusion relates his research to three basic problems of the period: the role of the Spanish in the activities of the League, the movements' ultramontanism, and the question of whether or not the League was genuinely "revolutionary" as some have claimed. Finally, he assesses the long-range influence of League political thought on subsequent French history—"very little"—and English history—a "small" contribution to the birth of modern liberalism—(pp. 240–42).

Two matters detract from an otherwise useful work. First, Baumgartner's style is sometimes flawed. For example, his use of the term "radical" is often muddied by adjectival qualifiers: "truly radical" (p. 9), "a rabid radical" (p. 73), and "a more truly radical Leaguer" (p. 75). And his inexact use of the phrase "the people" is sometimes confusing (see, for example, pp. 68, 192).

Second, Baumgartner asserts that French Leaguer thought had a significant impact on later English resistance theory. I am not convinced that this is the case; as Baumgartner himself admits (p. 242), its influence was only indirect and highly problematical. A careful reading of J. H. M. Salmon's *The French Religious Wars in English Political Thought* (Oxford, 1959)—which Baumgartner himself cites—reveals the perils of pressing this claim too far (see, Salmon, pp. 156–59).

Although there is little that is surprising in this volume, *Radical Reactionaries* is a welcome addition to a growing body of helpful studies of sixteenth-century French political thought. It covers the major League thinkers of the period well, sheds corrective light on the Spanish role in League policies, and fills in the gaps at a number of points. It supplants all preceding works on the subject and provides the most comprehensive discussion of League political thought in any language.

ROBERT D. LINDER
Kansas State University

STEVEN L. KAPLAN. *Bread, Politics and Political Economy in the Reign of Louis XV.* In two volumes. (International Archives of the History of Ideas, number 86.) The Hague: Martinus Nijhoff. 1976. Pp. xxx, 407; vi, 408–797. f 150.

Two hundred years ago an important public debate took place in France over a scarce commodity, vital to the very existence of some twenty million consumers. Was grain an "object of commerce" or an "object of administration"? Should the royal government stand back and let the forces of the market operate in the presumed long-run interest of producer and consumer alike, or should the state continue its traditional policy of pragmatic intervention and "police" of the grain trade in order to distribute this essential foodstuff as

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equitably as possible? This controversy, which engaged not only diverse intellectual schools, but also the makers and administrators of public policy between 1764 and 1774, is the subject of Steven Kaplan's massive study. In a class with a French doctoral thesis, the research is exhaustive and based on an intensive search through the enormous collections of administrative correspondence treating *subsistence* as well as on a thorough perusal of the writings of the *économistes* and other writers on the grain trade. What was it about this "debate" that justifies such extensive research?

Kaplan argues that liberalization of the grain trade in 1763–64 was much more than an aberration in French public policy, much more than a passing experiment in laissez-faire economics; it signaled a crucial stage in the relations of government to society. "The decision to renounce the old grain policy was the most radical and significant departure from the mold of the administrative monarchy made in the old régime" (p. 446). Although the policy was abandoned in 1770, Kaplan insists that its impact was profound, that the old assumptions about government responsibility for subsistence were now in question, and that the "crisis" of the decade was a harbinger of the "modern" spectator state. This debate also permits the historian to examine the articulation of various interests and ideologies in eighteenth-century French society that cut across many of the political and constitutional controversies, revealing fissures within institutions such as the parlements and groups such as the philosophes heretofore considered more cohesive.

Whether the "debate" marks a "crucial stage" on the road to "modernization" is a judgment with which one may take issue. But the significance of this research need not rest on this assertion. What Kaplan fully recaptures is the day-to-day operation of an administration grappling with a life-and-death problem. He is able to present precise profiles of such key administrators as Laverdy, Turgot, Joly de Fleury, and especially Terray, who emerges as a temporizing minister saddled with the unenviable task of picking up the pieces of a disastrous policy initiated by well-intentioned, but doctrinaire predecessors. Kaplan devotes considerable attention to the reactions of the parlements, whose diverse responses did not all reflect immediate economic and political interest; he is convinced that the Paris Parlement, for example, had a genuine concern for public order and for the plight of the capital's marginal consumers. He also shows how local administrators, baffled by their new role under a laissez-faire system, often deviated from the official policy of the central administration; "police" in many cases joined the "people." The problems of the grain merchants as procurement agents of the crown are evoked in specific case histories—Pascaud at La Rochelle, Guys at Marseilles—and do much to mitigate the harsh contemporary stereotype of the greedy hoarder and speculator. The anonymous consumer is inevitably more blurred, and here Kaplan must survey grain riots and public disturbances that filled the pages of the intendants' reports in the late 1760s. Kaplan has recreated the world of the public administration from top to bottom, an administration in the process of unplanned expansion in the techniques of bureaucratic *contrôle*. For at the end of the dec ade, the kingdom seems more centralized than ever, a confirmation of Weber and Tocqueville more than of Adam Smith, or so it seems to me from the "thick description" Kaplan presents.

Given the sheer bulk of this work, the author's allusion to yet unexplored aspects of the question suggests an excess of modesty. Kaplan's failure to investigate—to his own satisfaction at least—such issues as the anti-physiocrats after Galiani, farm leases in the 1760s, the victuallers' world, or the precise regional consequences of de-liberalization are surely forgiven. Yet one may still ask if this decade-long controversy on the grain trade in France requires seven hundred pages of text. No doubt the reader has a real sense of sitting beside Kaplan as he peruses each dispatch on the desk of the Controller-General and as he records each nuance of viewpoint from the interested liberalism of the magistrates of Toulouse to the hesitancies of the intendant of Orléans. My own view is that a leaner work would have enhanced the focus of argument and the elegance of exposition without sacrificing the exceptionally high quality of thoroughness and empirical research that the two volumes represent. "One does not need to exhaust the subject to know it," to quote Maurice Mandelbaum once again. But here are materials from thirty-five departmental archives and from all of the major depôts of Paris; what French historian could condemn out of hand Kaplan's dogged refusal to abandon a few shoeboxes of notes in the attic? The fact remains that this study can take its place beside those of Douglas Dakin and John F. Bosher as an outstanding contribution to the history of public policy and administration in Old Regime France.

ROBERT FORSTER
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MELVIN RICHTER, editor. *The Political Theory of Montesquieu*. New York: Cambridge University Press. 1977. Pp. xi, 355. Cloth $17.95, paper $6.95.

In this book Melvin Richter offers a collection and translation of some of the principal political writ-

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ings of Montesquieu, a hundred-page introduction, and a thorough set of notes. The selections include the Troglodyte and seraglio sequences from the *Persian Letters,* five analytical chapters from the *Considerations on the Causes of the Romans' Greatness and Decline,* and a representative selection of chapters from *The Spirit of the Laws.*

The introduction concentrates heavily on Montesquieu as political theorist, setting him mainly in the tradition of great political philosophers from Plato to Tocqueville. Richter's treatment is judicious and evenhanded, emphasizing the complexities and ambiguities in Montesquieu's writings. He sensibly rejects the popular interpretation of Montesquieu as simply an aristocratic ideologist. He emphasizes the multiple strains of thought—various natural law traditions, sociological insights, hatred of despotism, Enlightenment reformism, aristocratic traditionalism—which Montesquieu attempted, with mixed success, to present and reconcile. At times Richter's sense of evenhandedness and complexity betrays an unwillingness to decide among interpretations. Still the introduction serves a useful pedagogical purpose, opening up the varieties of interpretation to the reader without prejudgment.

The translations are clear and accurate. Richter has done a notable service in pointing the way toward a new English translation of the *Esprit des lois,* still available in its entirety only in the outdated eighteenth-century Nugent translation. Such a project must, however, await a critical edition of the original.

This collection will, therefore, offer nothing new to the specialist but can serve as a valuable introduction to Montesquieu for the general reader or for students in courses on the history of political thought.

HARRY C. PAYNE
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A. OWEN ALDRIDGE. *Voltaire and the Century of Light.* Princeton: Princeton University Press. 1975. Pp. xii, 443. $20.00.

A. Owen Aldridge has attempted to recreate the whole of Voltaire's life and thought by drawing upon the results of recent monographic studies and combining them in a biography which makes use of the methods of comparative literature and the history of ideas. The text presents a comprehensive overview of Voltaire's life which, through the effective use of lively quotations from correspondence and memoirs, succeeds in painting a very satisfying picture of the personality of a courageous, intelligent, vain, and fallible philosophe.

Aldridge has cast his net extremely wide, so wide that his study is more praiseworthy for its breadth than for its depth. The promise to combine the methods of comparative literature and the history of ideas does not produce so much an illuminating synthesis as it does an amalgam of observations on Voltaire's thought and literary style, elements serially arranged without being effectively combined. For instance, he points (following Ira Wade) to Voltaire's increased use of prose in the place of poetry, but there is no significant discussion anywhere in the text of the mutual interaction of Voltaire's ideas and his developing taste in literary style and form. Elsewhere he mentions that Voltaire's historiography was based upon his understanding of tragedy, a point of considerable interest which is noted without being developed. There are also some regrettable omissions. The *Traité de métaphysique,* a work which Aldridge describes as "the source for many of the philosophical ideas featured in the miscellaneous works of his later career" (p. 170), is almost completely ignored.

The cursory character of the treatment of Voltaire's thought occasionally results in unclear or questionable interpretations. If, indeed, Voltaire's political ideal was an enlightened monarchy, then reference to him as a "republican" or as one holding "much vaunted republican principles" demands elaboration. In some cases Voltaire is depicted as holding views which are contradictory while the contradiction is neither noted nor explained. In the *Dictionnaire* article, "*Chaine des êtres crées,*" he held that there is neither gradation nor continuity in the universe (p. 326). A few pages later (p. 331) Aldridge discusses *Le philosophe ignorant* in which the argument is that everything in the universe shows unity or design. The tension between these contentions would seem to require some comment on the development and coherence of Voltaire's philosophical views. On the important question of Voltaire's attitude toward the Jews Aldridge insists that his protagonist was not anti-Semitic. Yet, without citing Voltaire's harsh comments on Jews in his *Sermon de cinquante,* Aldridge criticizes Rousseau for daring to mention it: "it was definitely a disservice to Voltaire even to draw attention to such a work, let alone to say that he had written it" (p. 321). Defensiveness such as this is bound to arouse suspicions. No one can deal adequately with this topic without considering the argument and texts presented in Arthur Hertzberg's *The French Enlightenment and the Jews* (1968), a work which is unfortunately absent from Aldridge's bibliography.

In brief, this excellent introduction to the personality of Voltaire has important limitations as a guide to his thought.

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JAMES K. KIESWETTER. *Etienne-Denis Pasquier: The Last Chancellor of France.* (Transactions of the American Philosophical Society, volume 67, part 1.) Philadelphia: The Society. 1977. Pp. 190. $15.00.

Etienne-Denis Pasquier must hold a record of survival through French revolutions and of emerging in high place excelled only by that of Talleyrand. Born in 1767 into a family of the robe, he became in 1787 a councillor of the *Parlement de Paris*. He lost that position in the Revolution but survived the Terror, and in 1806, having come to terms with Napoleon, he became a *maître des requêtes* in the Council of State and four years later Prefect of the Paris police. In the spring of 1814, his loyalty to the Bourbons revived, he accepted Louis XVIII's appointment as *Directeur-général des Ponts et Chaussées*. He failed to re-enter the service of Napoleon in 1815 only, it appears, because Napoleon would not have him. This stroke of luck enabled him to return easily to the Bourbon fold a few weeks later, and in the next six years he served successively as Minister of Justice and acting Minister of the Interior, President of the Chamber of Deputies, Minister of Justice again, and Minister of Foreign Affairs. In 1821 he fell out with the Ultra-Royalists and left the ministry, but he continued to be politically active as a member of the Chamber of Peers. In August 1830 he accepted from Louis-Philippe the presidency of the Chamber of Peers and in 1837 the additional office of Chancellor of France, and he held both posts until 1848. He spent his remaining fourteen years in retirement, but there is some evidence that he sought unsuccessfully an appointment from Louis-Napoleon.

The reasonable expectation that such a career would be an interesting and valuable subject of a biography is not fulfilled, I regret to say, by James K. Kieswetter's book. The primary reason, I think, lies less with the biographer than with the subject. Pasquier is simply not an important historical figure. He was a trimmer, never a mover and shaper. Had he never lived, the history of France would not have been appreciably different. A second reason probably lies in the author's decision to write a purely political biography. Kieswetter is concerned almost exclusively with politics at the highest level, the frothy surface of history. His Pasquier moves in a rarefied atmosphere not only remote from the lives of most Frenchmen but apparently untouched by the forces and ideas that were shaping France's future. Moreover, what may be the most interesting aspects of Pasquier's life remain unrevealed and unexplained. The most extraordinary part of his career was his survival, and one wants to know what qualities he had that enabled him to survive and to flourish in a world in upheaval. What ambitions drove him? What fears restrained him? What passions diverted him? Perhaps the nature of the surviving sources inevitably confines a biographer within the narrow limits that disappoint in Kieswetter's book. If that is indeed true, the profession would have been better served by a perceptive biographical essay of perhaps fifty pages. Surely no more would be required to relate the essentials of Pasquier's career, place him in his times, and assess his significance.

DAVID H. PINKNEY
University of Washington

PETER N. STEARNS. *Old Age in European Society: The Case of France.* New York: Holmes and Meier. 1976. Pp. 163. $16.00.

Peter Stearns describes his *Old Age in European Society: The Case of France* as a "pilot effort" (p. 8) in the social history of the elderly as a group. The book focuses exclusively on France and, in spite of the title, makes no claims about European society as a whole. Yet in dealing with the important problem of old age in modernizing society it is establishing guideposts for future work. The book contains much new and useful information on hitherto unexamined topics such as the life-style of the elderly, residence patterns, geriatric medicine, and the role of unions in determining social responsibility toward the old. The study's strongest contribution is in the delineation of class attitudes toward old age in retirement, behavior, and lifestyle. The difference is clearly presented: "Whereas large segments of the middle class began to learn to prepare for aging in new ways during the nineteenth century, the working class did not" (p. 42). Members of the middle class were in the lead in the nineteenth century in formulating an active concept of what retirement should be. Stearns finds the explanation for these class differences in "middle-class wisdom" (p. 151), which emphasized control over one's own fate. The recurrent theme of this study is an optimistic one which stresses that modernization combined with the growing consciousness and initiative of the elderly can bring with it a new realization of age as a stage of life, instead of a deterioration, and can also bring with it the discovery of new, beneficial ways of dealing with this stage.

Working from a shaky statistical picture which is plagued by "imprecise variables" (p. 126), Stearns sees two main facts emerging: first, an increasing urbanization of the elderly, modified over the last three decades by a sun-seeking middle-class group; and second, a growing independence of the elderly vis-à-vis family relations. In both cases these facts are viewed primarily as elements of choice rather than as reflections of structural changes in the family and society. We

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are repeatedly told that the old are increasingly bent on shaping their own lives. This emphasis on initiative, while admirable and in certain cases accurate, goes too far in picturing the old as the masters of their fate in modern industrial society.

One of the best examples in the book of the inordinate stress placed on the idea of self-realization can be found in the brief, descriptive chapter on women. Elderly women are crucial in Stearns' analysis of the movement toward a new life-style for the old. On one hand, the problems of old women are seen as an exacerbation of the problems of old age in general: women are considered "old" earlier than men. On the other hand, women are in the vanguard, leading the way "through individual initiative" (p. 123) in determining a new and more optimistic life-style. The initiative of old women seems to be evidenced by their increasing use of cosmetics, face-lifting, hair-dyeing, and exercising. We are told that older women have felt better and looked better since World War II and this is explained on the basis of choice. Such an analysis is riddled with difficulties. No comparison is made with behavior of women of other age groups. Surely, the increased concern with physical appearance is important, but why should we interpret that as resulting from the self-determination and initiative of older women rather than the admittedly futile pursuit of standards of youth highly valued in French culture in particular and Western culture in general? Women, it is also claimed, adjust better to old age since they feel "less functionless" than men. One reason given for this is their ability to find solace in the mundane tasks of woman's work to which they are accustomed. The point here should not be that retirement creates fewer pangs for women because of their initiative, but that women are regarded as "functionless" longer and that their public function is either nonexistent or, in the case of working women, less highly valued.

Establishing how the old lived, what they thought, and what were their shared attitudes proves to be a highly problematic task for Stearns. He recognizes the old as the most inarticulate of groups in industrial society. As a result, Stearns is thrown back on examining the attitudes of doctors and popularizers who, Stearns acknowledges, probably did not have great impact on patterns of behavior in the nineteenth century. Instead, he chooses to argue that changes in medicine and literature reflected new behavior patterns generated by the old, especially the middle-class elderly in such areas as sexuality.

This study makes clear what future work is needed in writing the history of old age in European society. In the first place, more information is needed about the status and behavior of the old in preindustrial society. Stearns makes much of his case about the relationship between old age and modernization on the basis of an intuition regarding the place of the old in traditional culture: "the old were a nuisance" (p. 21). Secondly, the old should be studied in relation to the changing structure of the modern family rather than as a separate and autonomous age group. Finally, more concern for the physiology of old age and its social consequences is required.

This is an important work that recognizes the significance and relevance of the elderly as a group for the social historian. Neither the intention nor achievement of Stearns' work is to give a precise, detailed, and sophisticated analysis of old age in European society. Instead its goal is to point the way to other historians who are joining him in jumping on "the gerontological bandwagon" (p. 10). The importance of the work is not in the answers it gives but in its ability to raise questions and to suggest new approaches.

PATRICIA O'BRIEN
University of California, Irvine

PAUL WARWICK. *The French Popular Front: A Legislative Analysis*. Chicago: University of Chicago Press. 1977. Pp. ix, 211. $16.00.

For his re-evaluation of the Popular Front, Paul Warwick has conducted a statistical analysis of two major sources, a random sample of roll-call votes taken in the Chamber of Deputies and the electoral statements (*professions de foi*) made by victorious candidates in the 1936 elections. Arguing that these materials have been used only superficially in the past, the author suggests that a systematic quantitative analysis should cast new light on the Popular Front's disintegration and on the broader question of the decline of the Third Republic. Three appendixes discuss the statistical techniques employed, and throughout the book Warwick has mercifully "translated" into straightforward prose the conclusions drawn from the many tables and charts that accompany the text.

Warwick's sampling of votes reveals an interesting continuity in support for the Popular Front from the first Blum government through the later stages under Radical leadership. Normally, voting was characterized by two reasonably cohesive blocs reflecting loyalty or opposition to the Popular Front's goals. The chief defectors from bloc solidarity were a group of Radicals who opposed certain of the Front's social and economic policies. In analyzing the electoral statements Warwick discovered that most of the voting defectors were ideologically opposed to the reform goals of the

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Popular Front from its inception. Having isolated the Radicals as the weak link in the coalition, he tests what he terms the "hypothesis" of Peter Larmour (*The French Radical Party in the 1930s*)—that anti-Communism within their ranks was the "sole source of Radical defection from the Popular Front" (p. 132). Warwick finds that anti-Communism was very important, but argues that "it by no means stands alone" in explaining the disintegration of the coalition, and he suggests that anti-Communism was "closely related to the advocacy of a deflationary economic strategy" (p. 132). Actually, Larmour's interpretation is more subtle than Warwick indicates. Anti-Communist rhetoric cloaked a variety of grievances, and one doubts that Larmour would be surprised that anti-Communists also supported deflationary economics.

If not anti-Communism alone, Warwick believes that a basic ideological rift was the key to the Popular Front's demise. Vote defections "were not merely the result of Popular Front policy failures . . . but rather reflected ideological divisions within the French Left" (p. 137) which were so serious that "the Popular Front was fated to have difficulties even before it took office" (p. 138). While he seems to be aware of the importance of other factors (failures in economic and financial policy, the Spanish Civil War, and so on), the author insists that "the fundamental ideological cleavage" was "much more significant" (p. 164).

Despite the originality of the author's approach, this study does not provide a major contribution to our knowledge of the Popular Front. That the victory of the Popular Front did not represent a massive shift in public opinion or the basic composition of the Chamber has long been established. Georges Dupeux (*Le Front Populaire et les élections de 1936*) has demonstrated convincingly that major differences between the Radicals and their Marxist partners were evident before the Popular Front took office, and no one would deny the importance of their ideological conflicts. But why did the Radicals shift away from support for the coalition when they did and thus effectively destroy the Popular Front? A systematic demonstration of the existence of ideological differences (of which we were already aware) does not satisfactorily account for this process.

JOHN F. SWEETS
University of Kansas

TEMMA KAPLAN. *Anarchists of Andalusia, 1868–1903*. Princeton: Princeton University Press. 1977. Pp. xvii, 266. $13.50.

Most recent studies of Spanish anarchism have focused on the political vicissitudes of the movement, while relatively little work has been done on the social and economic bases of anarchism, particularly in the rural south. The present book, by Temma Kaplan, is a detailed and innovative analysis of the political evolution of anarchism in northern Cadiz province that relates changes in tactics and organization to more basic changes in the distribution of social and economic power in the region. Using evidence from municipal and provincial archives and from the liberal and anarchist press, the author convincingly argues that Spanish anarchism was a "rational, not a millenarian response to a specific social configuration" (p. 11), whose strength lay in its close identification with working-class needs and aspirations.

Both republicanism and anarchism in the province arose in protest against the liberal Spanish state, which sacrificed the interests of consumers and wine exporters to those of latifundist wheat growers. United through the revolution of 1868, the alliance of sherry merchants, small producers, skilled laborers, and agricultural workers soon broke down over issues of local control and social revolution. After 1869, the anarchists, led by peasant proprietors and skilled craftsmen whose prosperity and autonomy were eroded by the declining market for luxury sherry wines after 1863, rejected all cooperation with bourgeois politicians, seeking instead to mobilize the people through an organizational network that included unions, workers' centers, cooperatives, secular schools, and women's groups. Their goal was a revolution that would replace the existing bourgeois state with a free society of workers based on principles of equality and community.

Anarchism was successful in Andalusia because its democratic decentralization made it responsive to local conditions. Flexible and firmly rooted in popular culture, local anarchist groups survived underground even when state repression destroyed the national organizations. When political oppression eased, or when a favorable harvest gave agricultural workers leverage against their employers, the anarchist movement re-emerged with surprising strength. The periodicity of Andalusian anarchist uprisings should be seen as a calculated response to variations in political and economic conditions, not as the product of peasant millenarianism or inconstancy.

Of course, the strength of anarchism was also its weakness. Local control and individual autonomy could work against organizational solidarity, as Kaplan's insightful discussion of the split between "collectivists" and "communists" in the 1880s reveals. To combat this, anarchist theorists developed the doctrines and tactics of anarchosyndicalism, which aimed at uniting militant trade unionism with the interests of the community at large, including women, children, and the

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unemployed. In the agricultural towns of Andalusia, where the identity of "the people" and the anarchist movement was almost total, anarchism as a revolutionary theory retained its credibility well into the twentieth century. Nevertheless, the author might have added that, partly because of its commitment to democratic decentralization, anarchism reflected rather than combatted the inherent tendency toward fragmentation in the labor movement that was a result of the heterogeneous economic and social structure of Spain.

This is an important book, one which revises and deepens our understanding of grass-roots anarchism. Kaplan has shown that anarchist strength derived from its intimate relationship with popular culture. One hopes her book will provide the basis for further studies of the organizational activities of anarchists at the local level.

CAROLYN P. BOYD
University of Texas, Austin

JOSÉ AMODIA. *Franco's Political Legacy: From Dictatorship to Facade Democracy*. Totowa, N.J.: Rowman and Littlefield. 1977. Pp. 348. $15.00.

This book best contributes to the study of modern Spain by serving as a handbook for those trying to follow the rapidly changing events in that country. José Amodia's work suffers because it was finished before the death of Franco, but was not published until nearly a year and a half afterwards. It is primarily a work of political analysis, and frequently forecasts the political results of Franco's death—often erroneously, as we can now observe.

The author's aim is narrower than his title implies; he states that his primary concern is with the constitutional legacy of the Franco regime. Nevertheless, Amodia often strays from this narrowly defined area, especially to discuss the characters of some of the men around Franco and their potential roles in a post-Franco regime. Despite the almost journalistic flavor of some sections of the book (particularly a section entitled "The Opposition") the characters of both the pro- and anti-Franco factions remain shadowy.

Amodia's work is abstract, particularly in the areas of constitutionalism and political ideology. The author makes little attempt to relate theory to the realities of Spanish life, and his book therefore lacks the kind of historical perspective that might have made it more coherent and less rigid. He pays scant attention to the Spanish Left, and fails to differentiate sufficiently even among the various factions and phases of the Falange.

What does emerge from *Franco's Political Legacy* is the image of an *ad hoc* constitution that was constructed in answer to Franco's political needs rather than in response to any other factors in Spanish society. Amodia leads us deftly through the Spanish institutional maze that is at least partially a result of this haphazard development, frequently stopping to remind us of his personal bias against Franco and "Francoism," but always assuring us that the system is so well put together that only a cataclysm could change it.

Amodia recognizes no real alternatives to the continuation of "Francosim," nor does he give the reader even a hint of the variety and resources of the Spanish people on all levels which already appear to be leading Spain to a very different fate from the one he postulates.

The book is not without substantial value, however, for it succeeds in drawing a vivid portrait of the institutions of the Franco regime and how they evolved. Had the author used more of the historian's tools in uncovering and delineating the forces working against the continuation of Franco's system, however, his book might have had a better chance of surviving the era of change in which Spain is now engulfed.

PATRICIA ROOT FOUQUET
University of Nebraska, Lincoln

H. K. ROESSINGH. *Inlandse tabak: Expansie en contractie van een handelsgewas in de 17e en 18e eeuw in Nederland* [Domestic Tobacco: Expansion and Contraction of a Cash Crop in the 17th and 18th Centuries in the Netherlands]. (A. A. G. Bijdragen, number 20.) Summary in English. Wageningen: Afdeling Agrarische Geschiedenis, Landbouwhogeschool. 1976. Pp. 594.

This study of Dutch domestic tobacco cultivation is the most recent of the major monographs emanating from the Dutch Agricultural University's Department of Rural History. In contrast to the other studies—which were regional histories—this book presents the history of a crop. H. K. Roessingh's objectives are to uncover the processes of innovation and diffusion that controlled the spread of tobacco cultivation in the Netherlands and the interaction among market conditions, rural social structure, and cultivation techniques that conditioned the growth and contraction of the crop in the seventeenth and eighteenth centuries.

The North American reader might be forgiven for asking how important Dutch tobacco cultivation could have been; even the Dutch literature does not credit it with a prominent place. But it was for several generations a major crop, and Roessingh's detailed yet far-reaching study corrects many misconceptions, including several of importance to American colonial history.

Roessingh shows that Dutch tobacco production at its peak in the early eighteenth century equalled British re-exports to Europe of Chesapeake to-

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bacco. Moreover, the lower price of the Dutch product together with its close connections to the important Amsterdam processing industry made it, in the eyes of the British Board of Trade, a real threat to the then stagnant colonial industry.

How could Dutch tobacco, which was never grown on more than about 12,000 acres, compete with a colonial economy of vast extent? In a fascinating analysis, Roessingh traces the development of Dutch labor-intensive production techniques that achieved an output per acre at least thirty times greater than the Virginia yield, while the average product of labor in the Netherlands seems to have been only slightly inferior to that in the land-abundant New World. But Roessingh's calculations are based on *man-years*. Since he shows how Dutch tobacco farmers and their families poured their labor into their tiny farms, their output per *man-hour* surely fell far below that of their easygoing Virginia counterparts.

Roessingh sets out to account for the diffusion of tobacco in the context of social and market forces, but the dynamism of his analysis derives chiefly from the latter. The social conditions that encouraged poor cottagers in Utrecht and Gelderland to grow tobacco after 1650 are described, but they remain an exogenous, static factor. Yet, the social factor is of particular interest because the population of the tobacco areas grew while Dutch population as a whole declined.

This study gave Roessingh numerous examples useful in undermining widely held views about "innate" peasant conservatism; one could, therefore, have wished for a less oblique attack on what is a literature richly deserving rejection. But Roessingh's criticisms are muted, and, in one case less convincing than they could have been. He discusses the issue of innovational diffusion in the context of risk-aversion. The argument is not always persuasive, since the relative riskiness of tobacco cultivation is not clear, and the behavioral model appropriate to peasant risk-taking is open to question.

Like the other works published in the *A. A. G. Bijdragen* series, this is a study thoroughly grounded in archival research and rich in new information. It is also a well-focused study, sustaining a complex analysis which is, on the whole, convincingly argued. Roessingh has made a major contribution to Dutch agricultural history and has, in addition, contributed information and analysis that students of the colonial American tobacco trade ignore at their peril.

JAN DE VRIES
University of California, Berkeley

BERNDT FREDRIKSSON. *Försvarets finansiering: Svensk krigsekonomi under skånska kriget, 1675–79* [Financing the Defense: Swedish War Economy during the Scanian War, 1675–79]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 81.) Summary in English. Uppsala: Historiska Institutionen vid Uppsala Universitet, distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 159.

This is a well-organized book addressing important aspects of both economic and military history and resting firmly but not uncritically upon source materials scarcely touched in previous research. It is the more useful because it complements other scholarly work done in recent years on the economics of Sweden's seventeenth-century wars. It will not excite the general reader—even the six-page English "summary" would not keep one awake. But for the specialist Berndt Fredriksson's work will be a very welcome source of documentation. The financing of wars is vital to an understanding of them and has not heretofore received the detailed attention accorded the heroics.

In this work Fredriksson examines the provenience of the funds and supplies which made possible the defense of southwestern Sweden against Danish invasion in 1676–79. He describes all the resources tapped, from French subsidies to local requisitions. He indicates how anticipated revenues were converted (at a cost) into more immediately available money, food, or arms through loans and the operations of various entrepreneurs. The expedients employed by the government to meet pressing needs are well set forth. A significant section of the book explains the complex combinations of recruiting, conscription, and varying levies for obtaining and maintaining troops. The story does not, unfortunately, become "perfectly clear"; there are gaps in the source material which will leave some questions unanswered. One misses, for example, a real sense of the extent to which the population was impoverished by the exactions. The fact that "arrears" mounted tremendously and that the government adjusted and readjusted the tax base in an attempt to recognize ability to pay indicates that there was much hardship, but this study does not look at the revenue producers very closely. It is the revenue consumer whose records are examined, and these reveal that within the constraints of the situation the government made real efforts to spread the burdens of the defense effort. The nobility and other relatively affluent segments of the society were coerced into accepting heavy taxation—some of which they could probably not pass on to others—and even the inevitable war profiteers seem to have received at least part of their reward in "honors."

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California Institute of Technology

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DAVID GAUNT. *Utbildning till statens tjänst: En kollektivbiografi av stormaktstidens hovrättsauskultanter* [The Education of Officials in Early Modern Sweden]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 63.) Uppsala: Akademisk Avhandling; distributed by Almqvist and Wiksell International, Stockholm. 1975. Pp. 219.

There have been altogether too few Americans active in the study of Scandinavian history—with the exception of modern Finland; thus it is gratifying to note that, with the present *gesällprov* (a part of Uppsala's research project on the problems of the seventeenth-century Swedish state and society), the little band has increased. Using the technique of collective biography also applied to a not dissimilar topic in literary studies by Bo Bennich-Björkman (*Författaren i ämbetet: Studier i funktion och organisation av författarämbetet vid svenska hovet och kansliet 1550–1850* [1970]), David Gaunt has explored the archive of the court of appeals in Stockholm (*Svea hovrätt*). He seeks to analyze the social and educational backgrounds, attainments, and subsequent careers of some 850 auscultators attached to the *hovrätt*, from the foundation of the corps in 1627 until 1719; in other words, during the so-called Age of Greatness. (The appeals court at Jönköping, *Göta hovrätt*, is used only for comparative purposes, and that for Finland, *Åbo hovrätt*, not at all—the latter a regrettable but understandable omission: much of the court's archive was destroyed during the great Åbo fire of 1827.)

Gaunt begins by describing the background of the system. It dawned on Gustav Adolf that he had very few trained administrators at his disposal: Uppsala university supplied pastors—and even here, in the king's opinion, few were competent—but not enough civilian officials to administer the country, let alone Sweden's far-flung armies. The originator of the system (which provided on-the-job legal training) was the university's chancellor, Johann Skytte. His utilitarian pedagogical plans (influenced by Ramism) were transformed, to an extent, by Axel and Gabriel Gustafsson Oxenstierna: the brothers, fearful lest administrative knowlege—and power—slide too much into the hands of commoners, made the new service attractive for noblemen by seeing to it that bluebloods were paid, while others were not. The non-noble element in the corps nonetheless became and remained much the larger of the two. Gaunt plainly believes that the Swedish arrangement (as compared to related institutions on the continent) was quite beneficial to the nation, providing both experienced personnel and a healthy mix; the corps became neither a stronghold of the nobility nor the breeding ground of an anti-noble bureaucracy, and both elements were equally loyal to the crown. Of course, noblemen advanced quickly to high posts, while non-nobles often had to be content with employment in town administration throughout Sweden (a blessing, because of the practical skills they had acquired and their well-honed sense of responsibility). But the *ofrälse* were not excluded from any type of office, and, as well, not infrequently became themselves ennobled: "The [period of] Swedish absolutism became a time of break-through for a meritocracy" (p. 114).

Whoever reads Swedish but lacks the time to go through the entire book should pay particular attention to chapter two, on the origin and development of the system; four, on the recruitment of its members; and six, on "Some Characteristics of the Swedish Administration." Whoever has not mastered Swedish can figure out the numerous (and quite fascinating) charts with the aid of the English summary—which, surprisingly, is marred by misspellings ("manditory"), misprints ("a nigh noble drop out"), and misusages ("When . . . changes in the university system *delayed*, new academies . . . were established").

GEORGE C. SCHOOLFIELD
Yale University

ERLAND HOFSTEN and HANS LUNDSTRÖM. *Swedish Population History: Main Trends from 1750 to 1970.* (Urval, number 8.) Stockholm: National Central Bureau of Statistics. 1976. Pp. 186.

Sweden has the best and longest series of continuous population statistics of any country in Europe. Starting in 1749, local population registers formed the basis of a remarkably advanced system of data collection and reporting. Annual births and deaths were reported for the calendar year on December 31, a census was taken at the end of every fifth year, and both vital and census data were recorded in five-year age groups. This makes it possible to calculate reliable national demographic rates earlier and more completely for Sweden than for almost anywhere else.

This book serves as a useful short introduction to the population history of modern Sweden and presents a large number of tables of the most important demographic rates. The first section describes the development of the national population since 1750, giving crude birth and death rates, age-specific fertility and mortality, marital fertility, marital status by age, and the age and sex composition of the population, either by decade or in five-year intervals. Statistics on external migration by age and sex are available after 1861; the second section presents these rates for each of the Swedish counties since 1860 to show geographical variation. There are appendixes on the quality of the data, a retrojection of the population back to 1700, and the

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projected effects of external migration on overall growth.

There is, of course, a mass of detail, but the major results can be summarized quickly. Between 1750 and 1970 the population of Sweden grew from 1.8 to 8 million. There was an almost classic demographic transition in the nineteenth century. Infant mortality started to fall around 1800, childhood mortality around 1860, and adult mortality about 1920. Although the crude birth rate began to decline around 1860, the age-specific general fertility did not fall until 1880, which is the real turning point. The often-repeated warnings about the unreliability of crude rates is underlined by the fact that the city of Stockholm had the highest birth rates, but the lowest age-specific fertility. One of the most interesting tables shows age-specific fertility for birth cohorts since 1735. There was a continuous decline in the fertility of women under age twenty until the cohort of 1850, and then a continuous rise until 1950 when fertility for women under twenty was twice as high as it had been two centuries earlier. External migration was negligible until 1850, out-migration was pronounced from 1850 to 1930 when 1.5 million left Sweden, and the trend reversed to net in-migration after 1930. Migrants were primarily between fifteen and thirty-five, and internal migration has been largely dominated by the attraction of Stockholm.

There is nothing particularly new or unexpected about any of this; it is the quality, detail, and availability of the data which is of interest. Since the stated purpose of the book is only to present a reliable series of population data, the authors deliberately restrict themselves to description rather than analysis. There are no hypotheses tested, no correlations, no analyses of causes. It is a convenient little compilation of good data, but it is left up to the reader to find uses for the material and explanations of the results.

LUTZ K. BERKNER
University of California, Los Angeles

ERLAND F. JOSEPHSON. *SKP och Komintern, 1921–1924: Motsättingarna inom Sveriges Kommunistiska Parti och dess relationer till den Kommunistiska Internationalen* [The SKP and the Comintern, 1921–1924: The Controversies within the Swedish Communist Party and Its Relations with the Communist International]. (Acta Universitatis Upsaliensis, Studia Historica Upsaliensia, number 84.) Summary in English. Uppsala: Universitetsbiblioteket; distributed by Almqvist and Wiksell International, Stockholm. 1976. Pp. 364.

In this rather long doctoral dissertation the author devotes much attention to two themes: factionalism within the Swedish Communist Party during its first three years of existence and the relations of the party to the Communist International. While this work will prove of interest mainly to specialists, even they will discover that this is hardly the last word on the subject. The study has merit because of its attention to details, both large and small, but the cryptic style frequently reads more like a carefully edited protocol than a book on what is otherwise a subject of some importance for students of Scandinavian history and international Communism.

Erland F. Josephson provides abundant evidence that the party in Sweden suffered from the same difficulties as its counterparts elsewhere in Scandinavia and on the European continent. The traditional leadership (Zeth Höglund) was challenged by other ideologues or activists. Pragmatists in the party—and not least Höglund—were particularly averse to propagating atheism in a country that was at least formally committed to the Christian tradition. They were convinced, with some justice, that an overt commitment to revolutionary propaganda among Swedish workers would be counterproductive. Höglund and his friends had to deal as best they could with constant internal opposition, mostly from the Left. The youth section was frequently in revolt, and spokespersons for women clamored for more attention; the very nature of the party (mass or elite?) was debated both openly and by innuendo; and the nature and control of the party press was a live and sometimes bitter issue. Behind it all was the fear by some and the hopes of others that the Swedish Communist Party would conform to the pattern dictated by the Executive Committee of the Comintern. It is little wonder that a party split occurred in the autumn of 1924.

In pursuit of his evidence Josephson has gained access to a great deal of unpublished material—most of it in the Labor Movement Archives in Stockholm and some in other public and private libraries and archives in Scandinavia. He did not receive permission to use the pertinent material housed at the Institute of Marxism-Leninism in Moscow, but more surprising is the fact that he was not permitted to examine the personal papers of Zeth Höglund, now in the custody of his daughter. He was equally unsuccessful in gaining access to the Nils Flyg papers. There is consequently a regrettable gap in the internal evidence.

In attempting to describe the persistent and growing estrangement in the party the author has provided an abundance of detail, but it is detail devoid of color. He renders few judgments and his heroes are without character. Even in a doctoral dissertation one might well expect some description of the brilliant Ture Nerman, some enlarge-

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ment on the character of the distinguished newspaperman and memoirist, Karl Kilbom, and certainly an analysis of the controversial Zeth Höglund. There is likewise no effort made to place this study in the larger context of Swedish politics. This was a period of burgeoning moderate Social Democratic strength which was to appeal far more to Swedish intellectuals and workers than the more radical and revolutionary ideology stemming from Moscow and the Communist International.

ALBIN T. ANDERSON
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HERMAN POLLACK. *Jewish Folkways in Germanic Lands (1648–1806): Studies in Aspects of Daily Life.* Cambridge, Mass.: M.I.T. Press. 1971. Pp. xvii, 410. $15.00.

Herman Pollack's important study contributes substantially to the social history of pre-Enlightenment Jewish society. This book treats popular culture and religious tradition in four main areas of Jewish daily life: the interior of the home and the outside ghetto neighborhood; birth, education, marriage, and burial of the individual Jew; clothing, food, and popular medicine; and customs relating to synagogue worship, the Sabbath, and other religious festivals. His extensive documentation covers almost half of the volume, which contains 191 pages of footnotes and annotated bibliography to 198 pages of text. Although these cumbersome minutiae are excessive even for the specialist in the field, they do provide a wealth of information about the published rabbinical and secular literature of the period: chapbooks, memoirs, books of household remedies, medical handbooks, cookbooks, local collections of customs (*minhagim*), popular ethical literature (*musar*), communal legislation, minute books, rabbinic responsa, and folklore literature.

The most interesting sections of Pollack's book deal with parallel customs practiced by both Jews and Christians. One example is the practice of jumping over a bonfire or exploding gunpowder as part of the *Purim* or *Simchat Torah* celebrations; this seems to be related to the similar German *Johannes Feuer* celebrations on Easter Eve. In this case, popular custom prevailed over the objections of rabbis (p. 174 f.). There are also many examples of popular nostrums, plants, and folk remedies employed by both Jewish and non-Jewish medical practitioners. Using cookbooks and menus, Pollack surveys the dietary customs of seventeenth- and eighteenth-century Ashkenazic Jews. He states that "some foods of the general German cultural environment were adopted and made part of the rabbinic dietary tradition" (p. 112).

There are, however, serious limitations to Pollack's research. He is not concerned with the diffusion, extent, or origin of customs, nor does he deal with the distribution and differences in customs between rural and urban areas of Jewish settlement. There is no attempt to correlate Christian and Jewish practices regionally within different legal and political entities of the Holy Roman Empire, thus making it difficult to distinguish or explain where the life-style of Jews approximated that of the Christian milieu and where it differed.

Furthermore, Pollack does not analyze the distribution of different customs among the social classes of the Jewish population; his failure to describe how customs differed in various Jewish classes and occupations is a major flaw. The eighteenth century was the heyday of the Court Jews and saw the emergence of substantial Jewish mercantile and banking houses. Since printed literature is readily available about Moses Mendelssohn, Rahel Varnhagen, the Itzigs, and the Ephraims, Pollack ought to have included a description of Jewish customs practiced by the wealthy. The absence of this information is unfortunate, since only then would Pollack's work have fully complemented the recent studies by Jacob Katz, Jacob Toury, Michael Meyer, and Monika Richarz.

Despite these weaknesses, Pollack has written an interesting and worthwhile volume about the domestic life of the German Jew, "the social life of anonymous Jewish individuals and their communal activities" (p. xiii). The book has sixteen well-chosen illustrations which are clearly reproduced and captioned. It also has an excellent analytical name and subject index.

SYBIL MILTON
Leo Baeck Institute, New York

WALTER GROSSMANN. *Johann Christian Edelmann: From Orthodoxy to Enlightenment.* (Religion and Society, number 3.) The Hague: Mouton. 1976. Pp. ix, 209.

Johann Christian Edelmann (1698–1767) was a peripatetic intellectual during the early German Enlightenment. His religious pilgrimage carried him from orthodoxy through Pietism to rationalism, in none of which was he to find peace. Through his publications and especially his autobiography, one can take a fascinating and revealing look at German religious and intellectual life of this period.

Edelmann began his career at the University of Jena as a student of Buddeus, an orthodox Lutheran who was somewhat sympathetic to Pietism. Although he was later opposed to the orthodoxy of

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his professors, Edelmann as a student was far more narrow-minded than his teachers.

Unable to afford the fees related to obtaining a masters degree, Edelmann sought and obtained a position as tutor to the children of an Austrian Protestant nobleman. During this period (1724–31) Edelmann's orthodoxy was weakened as a result of his friendly contacts with Roman Catholics and especially through his acquaintance with Pietists in Vienna. As a result, his views were becoming more and more critical of the aridity and lack of ethical effectiveness of the teachings of orthodoxy, and he found himself reflecting the moral rigor of Pietism. His reading of Gottfried Arnold had a profound impact, followed by a brief relationship with Count von Zinzendorf and the Herrnhut community.

Still searching for a sure vocation, Edelmann began publishing a series of articles entitled *Unschuldige Wahrheiten*, the bulk of which were issued from 1734 to 1736. This was a period during which he was becoming more and more involved with Pietists and spiritualists of various types, which substantially aided his move toward criticism of orthodoxy and intolerance. When this combined with his own proclivity toward rationalism, however, Edelmann found himself coming into conflict with the antirational element found in many spiritualist groups. This led to his major work, *Moses mit Aufgedeckten Angesichte*, a rationalist attack on the major dogmas of Christianity. In addition, he openly stated his appreciation of Spinoza, a dangerous proclamation even in the tolerant atmosphere of Prussia.

Edelmann's Enlightenment phase thus began with controversy and continued until his works were condemned and burned. He was allowed to retire to Berlin in 1749 only upon the condition that he never again publish.

Walter Grossmann is currently editing a modern edition of Edelmann's works, to which this biography can be seen as a companion volume. The book reflects an intimate acquaintance with Edelmann's thought, and effectively relates Edelmann to his intellectual and religious sources. The style, however, is frequently pedestrian and rarely catches the fire of the controversies in which Edelmann was engaged. Nevertheless, this work on Edelmann is both long overdue and a welcome contribution to the history of the German Enlightenment.

RICHARD L. HARRISON, JR.
Eureka College

DIRK BLASIUS. *Bürgerliche Gesellschaft und Kriminalität: Zur Sozialgeschichte Preussens im Vormärz*. (Kritische Studien zur Geschichtswissenschaft, number 22.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 203. DM 38.

The history of crime and punishment is rapidly becoming recognized as one of the most exciting areas of research in social history. Any work on this subject is bound to be welcome, and this short monograph about the problem of crime in early nineteenth-century Prussia is no exception. Readers should be warned, however, that they will not find much here about the origins, motives, and social environment of criminals themselves. In fact, the author specifically rejects an approach which concentrates on those accused or convicted of crimes. In line with what he terms "critical criminology," Dirk Blasius concentrates instead on the social institutions and mechanisms which define certain actions as crimes and label certain people as criminals. In particular, the author is concerned with the way in which tensions within the elites of early nineteenth-century Prussia were reflected in different attitudes toward crime and different strategies for dealing with it.

Blasius provides some useful statistics about the incidence of crime (strictly speaking, of criminal investigations) in Prussia during the 1830s and 1840s: not surprisingly, increases in theft were closely correlated with increases in the price of food. More important to the author, however, is the way in which authorities interpreted and handled the problem of crime. By and large he finds that lower bureaucrats dealt sympathetically with petty crimes committed by people in distress, while higher officials and landowners pressed for more rigid standards of law enforcement. A particularly frequent misdemeanor was the theft of firewood from forests, and Blasius shows how an older attitude which understood such acts as throwbacks to traditional communal habits gave way to a rigid definition of wood-taking as a crime against the landlord's property. This he cites as one example of the "repressive strategy" which characterized the revision of criminal law in Prussia between 1794 and 1851.

Many readers will find the chapter on prisons especially interesting. In Prussia as elsewhere an older conception of prisons as a source of cheap labor was replaced by a more rehabilitative notion of the prison's function. This new approach, however, was often highly dogmatic. One telling incident occurred in 1818, when government officials ordered that a rising tide of masturbation at the Werden prison be stemmed by the introduction of a more exhausting work regimen. Only with difficulty could local officials persuade their superiors that the proposal was both unrealistic and economically unsound.

The author's treatment of this incident is char-

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acteristic of the book as a whole: he uses it not so much to describe the criminals' lot as to illustrate differences of opinion within Prussian officialdom. This is consistent with his purpose, of course. Yet somehow it is slightly disappointing that a book about crime in Prussia should end up reading so much like yet another monograph about the Prussian bureaucracy.

CHRISTOPHER R. FRIEDRICHS
University of British Columbia

FRITZ STERN. *Gold and Iron: Bismarck, Bleichröder, and the Building of the German Empire*. New York: Alfred A. Knopf. 1977. Pp. xxiv, 620. $17.95.

This book has its own history: almost two decades ago Fritz Stern and David S. Landes began their joint research in the Bleichröder papers now at Harvard University, in the Banque Rothschild at Paris, and in the government archives of East Germany, Austria, and Hungary. The "short book, quickly executed" (p. 597) that they originally envisaged has grown into two major projects. In his study of the Bismarck-Bleichröder relationship, Stern has produced one of the most ambitious of recent books on German and European history in the second half of the nineteenth century. Landes, faced with other urgent tasks, has not yet completed his history of the Bleichröder bank.

In his *Bismarck-Bibliography* (1966) Karl Erich Born listed more than 7,000 works on various aspects of Bismarck's career, yet Stern's book is the first study on Bismarck's relationship to the banker, Gerson von Bleichröder. It breaks a long-standing silence surrounding the discreet collaboration between the statesman who dominated German and European politics for nearly thirty years and the great Jewish merchant banker who built a financial empire during the same decades. That the significant role played by the "German Rothschild" has remained obscure was partly Bismarck's doing; in the first two volumes of his celebrated memoirs he failed to mention even once the name of Bleichröder, despite their long and at times daily collaboration.

Stern's analysis of the relationship of gold and iron is, above all, a profound and wide-ranging case study on the relationship between money and politics, on "economically conditioned" power (in the Weberian sense), and on the exercise of political power itself. Bleichröder was important to Bismarck in many respects: as his private banker and financial adviser, he built up the chancellor's personal fortune; and his financial advice and services extended far into matters of public finance. Bleichröder, for instance, found ways and means to finance the 1864 war against Denmark as well as that against Austria in 1866. Bleichröder again was instrumental in arranging the transfer of the French indemnity after the war of 1870. Because of his far-reaching connections the banker could act as a confidential go-between between Bismarck and his domestic opponents such as the leader of the German *Zentrumspartei*, Ludwig Windthorst. Through the collaboration with Bleichröder Bismarck had an opportunity to tap an international information system of a very particular kind. By virtue of his intimate relationship with the house of Rothschild, Bleichröder had access to a communications network of financiers that sometimes worked, as Bismarck had to admit, more efficiently than the communication system of the German foreign service.

Bismarck profited from Bleichröder's intimate knowledge of what went on in the realm of high finance, while Bleichröder received in return invaluable insights into international political developments. Nevertheless, this somewhat delicate interdependence of Junker and Jew was far from being an equal partnership. As Stern points out, "the history of the Bismarck-Bleichröder relationship suggests the primacy of politics, not economics. Bismarck is dominant and Bleichröder is useful" (p. xvii). As an adviser Bleichröder won a position of enormous influence, but he remained outside the range of political decision-making.

Stern's history of the pre-eminent Jewish banker, who rose from modest beginnings to immense wealth and influence and became the first Jew in Germany to be ennobled without previous conversion to Christianity, also offers new insight into the development of German anti-Semitism. Bleichröder's incessant striving for social acceptance and assimilation to the Prussian aristocracy was successful only as far as he himself was concerned. The Jewish banker was useful and reliable and therefore tolerated, but racial prejudices remained unchanged. Bleichröder's dramatic rise to social prestige and financial power even strengthened anti-Semitism, for he appears to be the prototype of the Jewish plutocrat. Finally, Stern's study throws light on the traditional and emerging power elites during the modernization crisis in Imperial Germany. Thanks to its unique and in most cases hitherto unused sources, the book illuminates many new and detailed inside aspects of politics and society in the Bismarck era.

During the years in which Stern worked on this book a methodological reorientation began in German historiography, producing a major reinterpretation of the relationship between economics and society in Imperial Germany. Although Stern comments on the development and present state of this debate, he was apparently unmoved by it. Be that as it may, his use of previously unknown

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sources, the subject matter itself, and Stern's brilliant style make the appearance of this book an outstanding event in German and European history.

KLAUS J. BADE
Harvard University

HANS-PETER ULLMANN. *Der Bund der Industriellen: Organisation, Einfluss und Politik klein- und mittelbetrieblicher Industrieller im Deutschen Kaiserreich, 1895–1914.* (Kritische Studien zur Geschichtswissenschaft, number 21.) Göttingen: Vandenhoeck and Ruprecht. 1976. Pp. 464. DM 84.

The *Bund der Industriellen* (BDI) was one of Wilhelmian Germany's best-known pressure groups. This book demonstrates that it was also ineffective. Conceived as a mass organization, it maintained the impression of dynamic growth only by padding membership figures. Financially dependent on a mass membership it never achieved, the Bund was permanently impoverished. Although it borrowed tactics from the *Bund der Landwirte* and aimed to displace the monopoly-oriented *Centralverband deutscher Industrieller* (CVDI), the BDI inexplicably renounced political activity for years. Instead, it submitted uninformative memoranda to bureaucrats in the innocent belief that they would impartially balance its claims against those of other groups. And when it entered politics after the turn of the century, it lacked the money to finance electoral campaigns and thus to obtain political commitments. The Bund's best hope lay in a federation of regional organizations; Gustav Stresemann's *Verband sächsischer Industrieller* of 1902 was highly successful, and similar groups were formed elsewhere, though not—and this was fatal—in Prussia. The regional groups, however, resisted full integration, and the Bund remained weaker than the sum of its parts.

The BDI also supplied unwelcome evidence for the Marxist notion that politics (at least interest politics) depends on conditions of production. The relatively homogeneous nature of coal, iron, and steel gave heavy industry common interests, but the diversified manufactures of the BDI's smaller, labor-intensive members obstructed cartelization, both economic and political. The Bund struggled constantly, and without lasting success, to reconcile the protectionist tendencies of its textile producers with the more liberal requirements of its exporting members.

Under Stresemann's prodding the Bund began to adopt new tactics around 1908, and after 1911 its fortunes improved somewhat. It courted political parties, mainly the left wing of the National Liberals, and tried to pressure the *Bundesrat* through its regional organizations. It appealed to public opinion on specific issues, sang the praises of risk-taking entrepreneurs (its own members), and bemoaned the bureaucratic vision of life, whether in government, industry, or labor unions. It advocated a variety of antisocialist labor tactics: yellow unions, separate insurance programs for white collar workers, the promise of high consumption in the future. And it strongly supported colonialism and *Weltpolitik*. Nonetheless, the BDI never approached the influence of the CVDI, especially in crucial matters like the drafting of tariff legislation. As late as 1913 the *Frankfurter Zeitung* called it the "*Bund der Unzulänglichen.*"

This monograph is broader than its title. In addition to the BDI, it also deals with the theory and practice of pressure groups, ministerial rivalries, "organized capitalism," and all tariff negotiations after 1895. Indeed, one has the impression that Hans-Peter Ullmann found the BDI too small a vehicle for his ambitions. His scholarship is vast—138 pages of notes for 230 pages of text—but the book is quite unreadable. Ullmann can write clearly, but his ideological analyses would make a Schoolman jealous. In addition, he approaches his topic from several perspectives—organizational, ideological, political—and in each idiom he repeats the history of the Bund. The book is a mine of information, but not a pleasure to read.

CHARLES MEDALEN
University of Arizona

EKKEHARD-TEJA P. W. WILKE. *Political Decadence in Imperial Germany: Personnel-Political Aspects of the German Government Crisis, 1894–97.* (Illinois Studies in the Social Sciences, number 59.) Urbana: University of Illinois Press. 1976. Pp. xv, 311. $10.95.

Wilke has rummaged industriously in a variety of East and West German archives and from these rich sources he has reconstructed a dense account of the battle for influence over Kaiser William II waged between 1894 and 1897. On one side was a conservative group consisting of Philipp Eulenburg, the Prussian cabinets, the royal entourage, and Bülow, and on the other Holstein and relatively liberal holdovers from Caprivi's "New Course" administration such as Berlepsch, Bronsart, Boetticher, and Marschall. The conclusion Wilke draws, which he notes is contrary to that advanced by John C. G. Röhl in his *Germany without Bismarck: The Crisis of Government in the Second Reich, 1890–1900* (1967), is that William II in fact did not succeed in 1897 in establishing a *persönliches Regiment* but instead had by that date become ensnared in the manipulative maneuvers of Eulenburg and his wily satellite Bülow. They intended to perpetuate an autocratic, antiparliamentary system in which William II

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would dutifully follow their counsel. The kaiser's captivity is presumably the "decadence" in Wilke's pretentiously ominous title; the use of the word, which does not appear in the text, is never explained or defended. To Wilke, the result of William's subjection to this reactionary clique was that imperial Germany failed to develop into a parliamentary monarchy, and this in turn paved the way for 1914 and subsequently for Hitler's ascent to power.

A good case could be made that neither Wilke nor Röhl has written the last word on the 1890s, for both labor under the assumption that there was a clearly discernible victor in the battle for influence. In fact, after 1897, as before, the conduct of government in Wilhelmine Germany was a contest of wills that never proceeded to the resolution which Bismarck had earlier accomplished at the expense of William I and all others who competed with him for power. While initiatives sometimes came from William II's servants in the government, German politics after 1890 was usually an exotic and unsettling farrago of royal caprice and bureaucratic adjustment. No one really led and no one, certainly not the kaiser's luckless subjects, won. With William II, nothing—including the alleged exercise of a *persönliches Regiment*—was steady and enduring. Wilke is correct in arguing that the Kaiser was temperamentally unsuited to be an autocrat. The royal will was spasmodic in application and inconsistent in its aims.

Wilke's narrative, which seldom transcends a textual analysis of letters and diary entries written by the major figures, constantly threatens to founder in billows of indigestible detail. He is concerned with people, but he almost never pauses to unravel the tangled roots of their connections with one another. The strange and powerful hold which Holstein exerted on Hohenlohe, Eulenburg, and Bülow needs to be explained in terms of their relationships while Bismarck was still chancellor. No illumination of the mutual fascination William II and Eulenburg had for one another is to be found here. We are provided little hint that part of Marschall's problems with the kaiser were due to the state secretary's abrasive personality and South German origins. Furthermore, the depiction of Hohenlohe, who drifted between the rival camps surrounding the throne, is unsatisfactory, for Wilke cannot decide whether the aged chancellor was a man of straw to whom politics was little more than temporizing or a selfpossessed statesman who shrewdly realized that ultimately more could be achieved by saying yes rather than no the kaiser. Wilke does better in describing the elusive and devious Eulenburg, but his critical relationship with William II is exaggeratedly described as being one in which the courtier "managed affairs" for his sovereign. In spite of all Eulenburg's unctuous and servile protestations of abject devotion to the Kaiser, he would indeed liked to have played such a commanding role. And later Bülow would be even more eager to place William II under his thumb. Neither succeeded, and both eventually were deserted by the kaiser, who in the end survived all those who sought to capture him for their own ends.

LAMAR CECIL
University of North Carolina, Chapel Hill

RICHARD J. EVANS. *The Feminist Movement in Germany, 1894–1933*. (Sage Studies in Twentieth Century History, number 6.) Beverly Hills, Calif.: Sage Publications. 1976. Pp. xiv, 310. Cloth $13.50, paper $7.50.

The title of this work is misleading. What Richard J. Evans has undertaken is an analysis of the bourgeois women's movement, as embodied in the *Bund Deutscher Frauenvereine* (BDF), not an analysis of the feminist movement as such. Though definitions might have helped clarify the intended scope of his study, the problem goes beyond one of semantics. For to label the BDF a feminist organization, even in its so-called "radical" period (1896–1908), clearly misrepresents the nature of feminism. Moreover, labeling the 1896–1908 period "radical feminist" corresponds in no way to the contemporary understanding of that term. The BDF was a bourgeois organization with middle-class aims, and this was its basic weakness. A minor criticism involves Evans' sexist word choice, including the use of masculine pronouns for undefined antecedents and the illogical reference to the *Vaterland* as "she."

These criticisms having been noted, what remains is an extremely ambitious and well-conceived study of the complex evolution of the BDF from its foundation in 1894 to its dissolution shortly after the Nazi takeover. On the basis of exhaustive research, Evans investigates the major traumas and turning points in BDF history, highlighting the 1908 rejection of liberal individualism in favor of a Naumannistic world-view which led the BDF down the road to political conservatism and *völkisch* pretensions. Indeed, the Nazi attitude toward women is foreshadowed in this evolution, providing a continuity in the German women's movement too often overlooked or misunderstood by students of the period.

Evans addresses two issues in particular which make this work of vital importance. Both concern the close relationship between the BDF and German liberalism and give this work wider impact.

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Fluctuations of BDF policy closely paralleled changes in the overall political climate. Like the liberals, the bourgeois women's movement leaders lacked a coherent ideology to give them direction and hence took a pragmatic position at every turn. While it may have ensured temporary success, in the long run this policy was as disastrous for German women as it was for German liberals. The dilemma each faced was the classic problem of representation versus leadership in the critical area of social legislation. Both the liberals and the BDF chose to represent rather than lead and found themselves in the position of being co-opted by both ends of the political spectrum.

In the larger context, Evans explores the fundamental weakness of the Weimar Republic. From the start, German liberalism proved incapable of accommodating change; in 1918 and again in 1933 a *deus ex machina* resolved conflict. Parliamentary action was a sham in imperial as well as in republican Germany. The women's movement reflects an important feature of the failure of liberalism in Germany and offers further insight into the nature of the Nazi triumph.

CARLA MESSMAN
Hamline University

GUSTAV STRESEMANN. *Schriften*. Edited by ARNOLD HARTTUNG. Introduction by WILLY BRANDT. (Schriften grosser Berliner.) Berlin: Berlin Verlag. 1976. Pp. xvi, 438. DM 38.

The number of monographs dealing with aspects of Stresemann's policies has increased rapidly since the war; the *Bibliographie Gustav Stresemann* compiled by Martin Walsdorff (1972) is 207 pages long! His own writings, while available to historians on microfilm, have long gone out of print. The most important selection of his papers, *Vermächtnis*, edited by his secretary Henry Bernhard in three volumes (1932–33), appeared at a most inauspicious time, just when Hitler came to power. He saw to it that the whole set was destroyed after only a modest number of copies had been sold; they are now rarely found on the antiquarian book market.

Arnold Harttung's compilation, therefore, fills a gap in more ways than one. Stresemann's *Reden und Schriften* (1926) stop after Germany's entrance into the League of Nations; the *Vermächtnis* starts, far too late, in 1923. The new selection, brief though it is, aims to cover Stresemann's whole life. Harttung avoids the error of overemphasizing his role in foreign affairs. Instead he does justice to his total personality, including his accomplishments in business organizations, social reform, and party politics. His smart selection is followed by a few testimonials written by people who knew Stresemann intimately such as Lord D'Abernon, Theodor Eschenburg, and Antonina Vallentin. The volume begins with an address by Willy Brandt and concludes with an impressive essay by Carl von Ossietzky, the pacifist martyr of the Hitler days; both, like Stresemann, were honored by the Nobel Peace Prize.

Any selection from such a wealth of material is bound to omit significant items. I regret, for example, the fact that Stresemann's moving tribute to Friedrich Naumann was not included, for Stresemann had learned more from him than from any other German political thinker. A table of historical events and a brief biographical dictionary at the end of the volume will be useful for younger readers in particular.

Harttung's volume should be especially valuable for American graduate students beginning research on Stresemann and the Weimar Republic. They will learn from it that serious study of him should not be limited to the period after 1918 or 1923, but ought to encompass his whole life and the many facets of his unique career.

FELIX E. HIRSCH
Trenton State College

JOHN HOWARD MORROW, JR. *Building German Airpower, 1909–1914*. Knoxville: University of Tennessee Press. 1976. Pp. ix, 150. $12.95.

The relationship between the military and industry has received considerable scholarly attention in recent years, but detailed investigations of the origins and development of particular branches of the armament industry have not been examined as carefully as they should be. John Morrow has written one of the first accounts in English of the development of the air industry in Central Europe before World War I. Based largely on archival research, Morrow compares the growth of the air industries in Prussia, Bavaria, and Austria-Hungary while noting the differences in the style and success these three states had in nurturing their infant industries.

In all three lands, important segments of the professional military quickly recognized the potential of the airplane and warmly encouraged its development, but the bureaucratic responses of their respective ministries were far different. Morrow argues that the Prussian policy of controlling aviation development through enforced industrial competition was superior to the Bavarian and Austrian policies of attempting to overdirect and guide their own selected, native factories. The Prussian War Ministry, although cautious, followed a supportive but sensible and pragmatic approach to-

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ward the industry in its embryonic years. The ministry was eager to help, encourage, and finance the industry within well-defined limits, but it insisted on competition. As a result, Morrow concludes, the Prussians succeeded in developing their aviation industry to a surprising level of sophistication before the war broke out. In particular, during the last two years of peace, they made great strides in catching up with the European leader in aviation, France.

By contrast, the Bavarian and Austrian ministries did not fare as well as the Prussians. Their lack of a basic commitment to aviation, chronic shortages of funds, and inability to rally civilian support for aviation meant that by 1914 they were unprepared for the new technological war. Morrow cites as decisive the impact of popular support for aviation in Prussia. For example, in 1912, a "turning point" in prewar German aeronautics, the public fundraising campaigns in Prussia pumped twice as much money into aviation as the Prussian army's budget.

This is an excellent, carefully researched and written monograph. Many striking photographs enhance the study. Scholars may find Morrow's thesis about the efficacy of competition unconvincing, but few will doubt the merit of his efforts.

EDWARD L. HOMZE
University of Nebraska, Lincoln

ALEXANDER KESSLER. *Der Jungdeutsche Orden in den Jahren der Entscheidung (II). 1931–1933.* (Beiträge zur Geschichte des Jungdeutschen Ordens, number 5.) Munich: Verlag Wolfgang Lohmüller. 1976. Pp. 336. DM 30.

This is the fifth in a series of five monographic studies on the history of the Young German Order during the Weimar Republic and represents the immediate sequel to Alexander Kessler's previous study covering the years from 1928 to 1930. A former member of the Order himself, Kessler does not attempt to write a critical history of the Young German Order, but rather to provide the discerning student of the Weimar Republic with access to documentation upon which an intelligent judgment of the Order and its political involvement during the last years of the Weimar Republic may be based. In this respect, the underlying intention of Kessler's work is to correct a widespread misconception of the Order as merely one of those countless *völkisch* organizations whose constant critique of "party-ism" and the various shortcomings of parliamentary democracy helped undermine the intellectual foundations of the Weimar Republic and prepare the way for the eventual triumph of National Socialism. While the Order's efforts from 1928 to 1930 to forge the German middle into a viable alternative to National Socialism may have ended in failure, there can be little doubt in light of Kessler's extensive documentation that the Young German Order remained from beginning to end one of the most determined and uncompromising opponents of the Nazi movement.

The real strength of this study, however, lies in its detailed analysis of the various internal crises which consistently plagued the Order following its ill-fated involvement in the founding of the German State Party and which greatly hampered the effectiveness of its campaign against Hitler and the radical Right. By making extensive use of the primary source materials on deposit in the *Jungdeutsches Archiv* in Berlin, Kessler is able to go substantially beyond the reconstruction of events contained in the standard monograph study by Klaus Hornung. In the final analysis, it is precisely in the wealth of its primary documentation—and not so much in its critical acumen—that the significance of Kessler's work ultimately lies.

LARRY EUGENE JONES
Canisius College

HELM STIERLIN. *Adolf Hitler: A Family Perspective.* New York: Psychohistory Press. 1976. Pp. 163. $9.95.

The author of this suggestive study, previously a staff member of the American National Institute of Mental Health, is presently Chief of the Division of Psychoanalysis and Family Therapy at the University of Heidelberg. Helm Stierlin draws heavily on his experience as a clinician to reconstruct Hitler's relationship with his mother. He believes that young Hitler fits a pattern of disturbed sons whose mothers force their children to serve as "bound delegates," burdening them with the responsibility of fulfilling their own frustrated aspirations, and condemning them to carry out "missions impossible."

Making Klara Hitler a central figure of this psychodrama—and, indeed, of European history—poses problems for the author. As he ruefully admits, specific data about Hitler's mother are indeed "sparse and questionable." We therefore have no direct knowledge about the "missions" she charged Adolf to fulfill. Stierlin argues, however, that in Hitler's psychodynamics his mother became the Motherland. Thus the Führer felt that he was fated to carry out the fondest desires not only of Klara Hitler but of the German people collectively.

Stierlin uses this formulation to explain Hitler's

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immense sense of mission and his special appeal to the German people. As the delegate both of his mother and of his Motherland, his mission was to avenge his mother's murder at the hands of a Jewish doctor by killing the Jews who sought to destroy the Motherland; and he would redeem his mother from shame and guilt (her illicit sex life with her uncle, Alois, Adolf's father) by redeeming Germany from an "analogous trauma," the defeat of 1918. Because the loss of Klara's children in early childhood became the loss of Germany's "children" through the Verailles Treaty, he would bring all Germans "home to the Reich." His harried mother also delegated to her son the task of resolving the conflicts of her family—and those of all Germans—by creating a harmonious superfamily, a mighty *Schicksalgemeinschaft*. Further, the orality problems and confined living space of Adolf's infancy would be rectified by supplying the whole German family with ample food and *Lebensraum*. Through his heroic exploits he would fulfill his drab mother's—and Germany's—need for excitement, power, and achievement.

Thus, Stierlin concludes, Hitler's entire *Weltanschauung* was "in accord with *her* wishes and visions"; her trauma was Germany's trauma, her redemption, Germany's redemption. And just as Hitler both adored his "sainted mother" and resented her, so he idealized and debased Germany. In the end, he sought to destroy his mother by destroying the Motherland he professed to love.

Admittedly, this is a speculative essay in which the author derives broad and intriguing conclusions from meager evidence. It must also be said that he tends to underestimate the complexities of his subject's personality and of German history. Nevertheless, Stierlin has made an original and eminently readable contribution to our understanding of one of history's most enigmatic characters.

ROBERT G. L. WAITE
Williams College

JOHN TOLAND. *Adolf Hitler*. New York: Doubleday and Company. 1976. Pp. xx, 1035. $14.95.

Adolf Hitler continues to fascinate readers and authors, a fascination justified by the role of one individual in decisions and events that affected the lives—and deaths—of millions and influenced developments on a global scale. There are three general categories of literature on Hitler and his Germany in English: an extensive scholarly literature including a few books of broader sweep like Alan Bullock's biography and Karl Bracher's *The German Dictatorship*; a body of sensationalized popular literature; and works designed for a wide public but making a serious effort to incorporate scholars' findings while adding the flavor of dramatic reconstruction, often based on extensive interviewing. Toland's massive biography of Hitler belongs in this last category.

Utilizing a substantial body of secondary works, published documents, some publicly and privately held archives, and interviews with relatives, friends, and associates of Hitler, Toland has written a popular biography. There is an extensive discussion of Hitler's family and early life, and the years in Vienna and Munich are reviewed at length and very well. A sketchy account of service in the army is followed by a longer record of Hitler's activities in postwar Bavaria. The Beer Hall Putsch and its consequences, the rebuilding of the National Socialist Party, and the ascent to power in Germany are succeeded by developments in the Third Reich before the war. More than a third of the text covers the war years, ending with a lengthy account of Hitler's last days, marriage, and suicide.

Hitler himself is the focus of the narrative. This has the advantage of highlighting the individual as well as his relations with those around him; the treatment of Eva Braun's role in his life is particularly well done. On the other hand, there is not much sense of the surrounding developments, and even very little analysis of Hitler's own purposes and actions. Toland quite properly gives much attention to Hitler's attitude toward the Jews and the priority of the extermination program, but in this as in most other areas, there are numerous errors. It is probably more useful to indicate categories of mistakes rather than listing a great many. The fact that the author is neither fluent in German nor well acquainted with German history has greatly affected the account; thus we are told that Hitler "paved the way to social security and old-age benefits" (p. 403) when these had existed long before Hitler set foot on German soil.

Another category of errors results from unfamiliarity with some critical research combined with uncritical dependence on unreliable books and persons. He accepts the Tobias thesis on the Reichstag fire as final, and accepts every tale concocted by Erich Raeder and most of the inventions of Fritz Hesse at face value. The former has been set straight by Gemzell and Dülffer; the latter was exposed by Helmut Krausnick more than twenty years ago. Where Toland cites recent scholarship, he often brings it in strangely, with theories about Hitler's ancestry or the financing of the party presented one way in the text, while the contradictory findings of scholars are relegated to footnotes (pp. 247, 277).

What is, moreover, misleading in the total picture is a dichotomy in the presentation. For the

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prewar period, we are shown a driven—rather than a driving—Hitler. He is pushed into the Röhm purge; he feels threatened by Czechoslovakia; he is really upset by the Poles—all most dubious constructions. Once war starts, Hitler is pictured as more assertive, though even then there are curious reversions to the earlier picture.

It is difficult to make a general judgment on the book. There are sensible interpretations, and there is often a sense of balance. The information from interviews occasionally adds knowledge and insight, but often produces distortions. All in all, Bullock is better.

GERHARD L. WEINBERG
University of North Carolina, Chapel Hill

DAVID IRVING. *Hitler's War*. New York: Viking Press. 1977. Pp. xxxiii, 926. $17.50.

Arguably the greatest challenge facing historians of the Third Reich involves applying the principles of scholarly objectivity to a system universally condemned both intellectually and emotionally by decent human beings. The assumption of the impossibility of a significant revisionist approach to the Nazi era has so far remained valid, at least on an academic level. *Hitler's War*, however, appears on its way to becoming something of a cause célèbre: critics are asking whether David Irving is not exculpating at least Hitler himself from some of the atrocities of his regime. Specifically, they focus on Irving's assertion that real responsibility for the Holocaust rested with the SS. Hitler's loathing for Jews, according to Irving, manifested itself primarily in fantasies. In concrete terms it went no further than plans for resettlement and orders for deportation. Himmler and Heydrich not only never received an explicit warrant for genocide, but systematically submitted false or misleading reports about the nature and extent of their activities. While Hitler was discussing exiling Europe's Jews to Madagascar, his fanatical subordinates were instead throwing the extermination machinery into high gear.

Irving's argument can be criticized on two levels. He overlooks, instead of refuting, Lucy Dawidowicz's argument that Hitler's language was Aesopian, concealing the reality of extermination beneath the rhetoric of expulsion. He also discounts the evidence that, however twisted the road to Auschwitz may have been, concrete decisions implementing the Final Solution were taken, and these decisions involved Adolf Hitler. Irving emphasizes the difficulty of establishing precise documentary connections between Hitler's anti-Semitism and the extermination of the Jews until the process was virtually completed. But while the Third Reich has been compared to every form of government from European feudalism to Oriental despotism, another analogy suggests itself—the Mafia structure as commonly visualized. On certain sensitive issues the Godfather's will is understood and implemented with little need for formal instructions!

The general thrust of Irving's work also contradicts his interpretation of the Final Solution. Unlike scholars who stress the *limits* of Hitler's power, Irving takes pains to show that the war was Hitler's war from start to finish. He overrode all opposition from the *Wehrmacht*, the party, or the foreign office. He alone knit together the threads of intelligence and strategy, production and allocation. In this context, even given the increasing autonomy of the "SS state," could subordinates have so completely concealed for so long the elaborate apparatus of the death camps from the Führer?

It would be unfortunate if *Hitler's War* were to be judged solely on its treatment of the Holocaust; Irving is neither a closet revisionist nor a covert anti-Semite. The key weakness of his book is professional, not polemic. It tends to push every bit of evidence to the limits of credibility, and the Jewish issue is only one example. Thus Irving presents Hitler's respect for Britain as generating so strong a determination to defeat it that he attacked Russia, hoping to strip Churchill of two potential allies with minimal risk. Russia would be destroyed; Japan would push her claims in the Pacific to the point of drawing the attention of the United States away from Europe. This argument in turn deemphasizes to the point of distortion both Hitler's ideological anti-Communism and his longstanding commitment to winning *Lebensraum* in the East. To cite further examples of Irving's approach would only belabor the point. *Hitler's War* is essentially a nine-hundred-page seminar paper of the kind often produced by intelligent beginning graduate students. It is a work too clever by half, and certainly too clever to be sound history.

DENNIS E. SHOWALTER
Colorado College

BRADLEY F. SMITH. *Reaching Judgment at Nuremberg*. New York: Basic Books. 1977. Pp. xviii, 349. $15.00.

In this well-researched study, employing previously unavailable or unutilized sources (particularly the personal courtroom diary of the chief American judge, Francis Biddle), Bradley F. Smith moves beyond the polemics of the past to show, in all its complications and complexities, how judgment was in fact reached in the Nuremberg trial of the Nazi war criminals.

The opening chapters analyze the background of

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the trial—the decision behind holding such a trial, how the charges came to be drawn up, and how the defendants were selected.

While these introductory chapters are valuable and important, the heart of the book is the trial itself. Smith demonstrates the substantial degree to which the judges were independent of both the prosecution and their governments. As a consequence of this independence, "the defendants faced a court surprisingly free of outside control" (p. 8). Beyond this, the judges were aware of the problems inherent in the charge of conspiracy to commit crimes against peace and in the implementation of charges against criminal organizations. In the end, eight of the twenty-two defendants were convicted on the charge of participating in a conspiracy to commit crimes against peace. And all eight were convicted on the second charge of committing crimes against peace. On the basis of this and other examples, it is reasonable for Smith to conclude that the judges' "attitude toward legal process and the situation's complexities were the decisive factors" (p. 76), rather than their bias against the defendants. His analysis of the deliberations on each defendant further substantiates the general validity of this judgment.

Smith does assert, however, that the Nuremberg verdicts revealed social prejudices on the part of the judges, that "those defendants who came from a milieu closely approximating that of the judges were the ones who received the most sympathetic treatment" (p. 305). But Smith resists the temptation to make too much of this point and he admits that ultimately the "results were due in large measure to the merits of the individual cases" (p. 305).

The Nuremberg court did not produce perfect justice. No court ever does. But the trial did prevent "an anarchic bloodbath" (p. 303) that would have occurred had Nazi leaders been subject to summary execution, which was initially favored by some highly placed Allied officials. And by finding some defendants more guilty than others and by acquitting three, the Nuremberg tribunal avoided the prospect of placing future "belligerent leaders in a situation where the only alternatives for them are victory or death" (p. 303). One must conclude, with Smith, that in answering the grave questions facing them, the eight judges at Nuremberg "did so in a manner that gives the rest of us greater cause to hope than to fear" (p. xviii).

This is a masterful and dispassionate study and one that will make it impossible for any fair-minded person in the future to dismiss the Nuremberg trial with scorn simply as "victors' justice."

BIRDSALL S. VIAULT
Winthrop College

ULRICH BORSDORF *et al. Arbeiterinitiative 1945: Antifaschistische Ausschüsse und Reorganisation der Arbeiterbewegung in Deutschland*. Wuppertal: Peter Hammer Verlag. 1976. Pp. 782. DM 48.

Two Germanies arose from the country that during the first half of 1945 was virtually reduced to the rubble which Hitler's "Nero order," though never executed, prescribed as a fitting end to the Third Reich. The recovery of this vanquished nation and the almost miraculous return of organized civic life crystallizing into two modern states has been studied in great breadth and detail. Yet the most chaotic period—from spring to winter 1945, when the whole German society appeared to be in flux—has escaped careful analysis and been generally subsumed under later developments. *Arbeiterinitiative 1945* is the cooperative effort of twelve young West German historians to explore those first formative months of reconstruction. The aim of these historians, whose orientation represents a spectrum of "left pluralism," is to correct the prevailing current in the historiography of postwar Germany, attributing the course of events to the activities of leading personalities. As the Allies marched into Germany, they argue, the Adenauers, Schumachers, and Ulbrichts were hardly significant. The only independent political force the victors encountered were diverse committees throughout Germany under the rubric *Antifas* and initiated by members of the former working-class movement. But it was not the trade-union, Social-Democratic, or Communist orientation of various initiators that joined the grass-roots committees into a national antifascist movement. Significant, the authors maintain, is the spontaneity of organization and attempted scope of activity of what was potentially an alternative to the systems that ultimately emerged in the BRD and DDR.

The work is organized in three parts. A substantial opening section reviews and analyzes the party and trade union Left in all of its sectarian complexity from the Weimar period through the years of emigration and resistance. Particular attention is paid to the variety of programs and proposals developed for the reconstruction of Germany after the defeat of fascism. The fate of the German working class after the antiresistance razzias of 1937 and under the impact of the war and final break-up of the Nazi regime has never been chronicled so well. The very large center section, really a series of small monographs, puts the organization and activities of the *Antifas* of Leipzig, Solingen, Ruhrgebiet, Hamburg, Braunschweig, Duisburg/Mühlheim, Bremen, Frankfurt, Hannover, and Stuttgart on record. As the Hitler regime crumbled, these committees arose spontaneously in residential quarters and factory districts and in a number of large cities assumed control of all municipal affairs. Regardless of their political orientation or composition, and despite the lack of

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contact between them, the form and activity of these committees was remarkably similar. For weeks and months they constituted the only organized authority carrying out the physical, political, economic, and social reconstruction of a totally disrupted society.

In the closing section the authors attempt to account for the short life of this alternative form of democracy from below—so reminiscent of the fate of the *Räte* movement of 1918–19. Their explanations—chiefly that the occupying authorities restored many of the old centralizing authorities and structures—seems insufficient, but as a first work on an important subject this book is a remarkable contribution to scholarship. At the very least it makes clear that the commonly accepted proposition that the German working class and its parties was brought to quiescence by betrayal and terror between 1933 and 1945 must now greatly be revised.

HELMUT GRUBER
Polytechnic Institute of New York

HELMUT MEYER. *Der Zweite Kappeler Krieg: Die Krise der schweizerischen Reformation.* Zurich: Verlag Hans Rohr. 1976. Pp. 479. 52 FR.

Reformation studies have long been dominated by theologians and intellectual historians. While this is appropriate for a movement in which ideas were so important, it is refreshing to note the increased attention scholars are now paying to the economic, political, and social dimensions of the Reformation. Historians have come to a fuller appreciation of the importance of establishing the context in which ideas originate and have impact. Swiss historian Helmut Meyer, by writing a detailed history of *Der Zweite Kappeler Krieg*, has made an impressive contribution to the diplomatic, military, and larger political history of the Reformation in Switzerland.

The Second War of Kappel is known primarily as the campaign which took the life of Ulrich Zwingli and consequently cost Zurich the leadership of the Protestant movement in Switzerland. Meyer shows us in meticulous and often fascinating detail that the war was this and much more. His work is especially noteworthy because it is based on intensive archival research, which sheds much needed light on an important topic that has not been fully studied before.

The first section of Meyer's book treats the background of the war. He examines the religious-political lives of the various parties to the conflict and the relations between them. Then he assesses the impact of the economic blockade against the Roman Catholic cantons, *Die Fünf Orte*, which did so much to bring about their surprise attack on Zurich, the leader of the Evangelical cantons. Especially interesting is Meyer's treatment of the roles of the "Secret Council" and Ulrich Zwingli in influencing Zurich's aggressive religious foreign policy on the eve of the war. He concludes that Zwingli was "not Zurich's uncrowned king," but that he was more influential than his counterparts in Bern, Basel, or Strasbourg (p. 88).

In the book's second section Meyer treats the military campaign and the resultant peace negotiations. Although his narrative is crisp and straightforward, a few military and political maps could have been used to good advantage. His concluding section looks at the impact of the war on Zurich and the other participants. Here, as throughout the book, Meyer does a fine job of placing Swiss events in their larger European perspective. He makes it clear why the war was a disaster for Zurich, especially when compared to the more cautious Bern. Meyer concludes that the religious truce achieved by the Swiss in 1531 compares with what the Germanies achieved in 1555 and the French in 1598. Even Zurich, which had suffered so much in the war, benefitted in the long run from the eventual development of a full policy of Swiss neutrality. This impressively researched and written volume deserves the attention of Reformation scholars and historians of Switzerland.

JONATHAN W. ZOPHY
Carthage College

BRUNELLO VIGEZZI. *Giolitti e Turati: Un incontro mancato.* In two volumes. Milan: Riccardo Ricciardi Editore. 1976. Pp. xxvii, 741. L. 20,000.

This intricate, suggestive, and massively documented study is the work of a scholar noted for his pioneer research in the period between the latter years of the Giolittian era and the advent of Fascism. Although Brunello Vigezzi does not pretend to offer either a history of Italian reformist socialism or a biography of its protagonist, Turati, he has attempted to clarify a number of questions regarding the reputed failure of reformism between 1909 and 1915, particularly in the context of the reformists' relations with Giolitti, the dominant political interlocutor of the times.

For Turati, the major problem was to preserve the autonomy of the Socialist party by preventing its engulfment in the Giolittian majority, and at the same time to follow a policy of socialist reformism—if possible in conjunction with Giolitti's professed liberal-democratic reformism—while holding in check the revolutionary currents in Italian socialism. The meaning of the "*incontro mancato*" (missed encounter) lies, then, in the failure to have

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Giolitti break with his amorphous personal majority that ranged from the Right to the democratic Left, and create a new political alignment, conspicuously of the Left, which the reformist socialists could support on the basis of a program of "grand reforms." According to Vigezzi, this policy failed largely because, despite Turati's appreciation of Giolitti's positive qualities and the occasional collaboration of the two men, more often than not they disagreed. Vigezzi denies that a consistent cooperative Giolittian-Turatian "system" ever existed and insists that the supposed convergence of Giolitti's democratic liberalism and Turati's reformist socialism is a "myth."

On two occasions, in 1903 and 1911, Giolitti offered the Turatian wing of the party the opportunity to assume its responsibilities in promoting the democratization of Italian society. Both times the encounter between Giolitti and Turati went astray. In the first instance, Turati's reluctant refusal to enter the government led to a long estrangement from Giolitti, during which time the socialists experimented with Sonnino and Luzzatti as alternatives to a much-feared Giolittian tutelage. And when, in 1911, Giolitti made his second offer, accompanied by a program of major reforms (nearly universal manhood suffrage, state monopoly of life insurance, and a comprehensive system of workers' pensions), it was Turati above all who vetoed this second and more significant encounter, the interpretation and consequences of which constitute so large a part of Vigezzi's work. How to explain this second rejection in the light of Turati's own verdict of 1908 that Giolitti was "the only serious statesman we have in the Chamber," who spoke as a veritable reformist, and, most important, "understood the laws of gradualism and the futility of attempts that anticipate too much the evolution of the capacity of the workers and of public opinion" (quoted p. 5)? The answer lies mainly in Turati's doubts that Giolitti had become "so democratic" in 1911 (quoted p. 79), especially in view of his reputation as "a political figure of little faith, lacking in ideas and programs," who could not, simply by waving "a magic wand," achieve a rapprochement between liberals and socialists (quoted p. 81).

In fact, a convergence was possible in 1911 when Giolitti presented his grand program, which the reformist socialists promised to support even after having refused to enter the government. But the Libyan war effected a revolution in Italian political life. Turati's uncompromising opposition to the war led to a break with his old collaborator, Bissolati, whose expulsion from the party in 1912 marked the triumph of the revolutionaries and the emergence of Mussolini as the new socialist protagonist. The war ended the reformists' renewed hopes of fruitful collaboration with bourgeois liberalism for the gradual transformation of Italian institutions. And while the rallying of the conservatives around the Libyan enterprise seemingly strengthened Giolitti's majority, its increasingly amorphous composition presaged his retirement from office early in 1914.

The outbreak of the First World War found Italy led by Salandra, a firm antagonist of Giolitti, who saw in the war the golden opportunity for a conservative resurgence in domestic policy supported by and in conjunction with a "patriotic" intervention on the side of the Entente which would lead to fulfillment of Italy's territorial ambitions in the Alps and the Adriatic. Inasmuch as both Giolitti and Turati opposed intervention, an encounter should finally have taken place. But the disarray in socialist ranks (whose complex divisions Vigezzi disentangles with a masterly hand), and the differences between Turati and Giolitti on the manner and purpose of keeping Italy neutral, facilitated Salandra's interventionist policy. The war that followed signaled not only the defeat of the Turatian and Giolittian varieties of reformism but also the eclipse of Italian liberal democracy.

Vigezzi's study is remarkable for the richness of the sources he has tapped. These include archival material, memoirs, newspapers and periodicals, virtually all relevant secondary works, and the correspondence between Turati and Anna Kuliscioff, his companion of four decades. It is above all in the letters between Turati and Kuliscioff, annotated by Vigezzi with unparalleled discernment, that we see revealed a Turati who, despite frequent doubts, hesitations, and discouragement, remained true to his vision of a reformist socialism that would prepare the working classes for full participation in the direction of a democratic Italian society without recourse to counterproductive revolutionary violence. In pursuing this vision, Turati was aided by the clear, searching, and at times unsparing intellect of Kuliscioff, which emerges from Vigezzi's account as a major force in shaping Turati's thought.

If the Turati-Kuliscioff reformism failed to direct the course of Italian socialism during the several years before 1915, and missed the possible encounter with Giolitti, the double failure nevertheless stands as a lesson to the current arbiters of Italy's political life.

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MICHAEL A. LEDEEN. *The First Duce: D'Annunzio at Fiume*. Baltimore: Johns Hopkins University Press. 1977. Pp. xi, 225. $13.50.

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Michael Ledeen attempts a major revision of our view of D'Annunzio and the Fiumian adventure in which the poet becomes "one of the great innovators and the occupation of Fiume one of the watersheds of the modern world" (p. x). Ledeen argues that, taken on its own terms rather than as merely a foretaste of Fascism, D'Annunzio in Fiume offered a fascinating excursion into the psychology of mass politics.

The author outlines two major periods of the adventure. The first, from September to December 1919, was dominated by conservative nationalists who aimed at the overthrow of the Italian government of Francesco Nitti or, failing that, at the liquidation of the affair on terms favorable to the Right. This period ended when D'Annunzio's followers among the radicalized veterans refused to allow a referendum in Fiume on a settlement worked out between the government and nationalist politicians.

The major contribution of this study comes in the new light shed on the neglected second period of 1920 when the leftist supporters of the Fiumian cause had their day. Men like Giuseppe Giulietti, the head of the seamen's union, attempted to interest the socialists in a revolutionary "march on Rome"; Guido Keller, D'Annunzio's "action secretary," tried to instill a sense of revolutionary activism by a series of daring strokes outside the city; and Alceste De Ambris, a syndicalist, sought to institutionalize the revolutionary potential of the adventure in the *Carta del Carnaro*, a constitution which combined syndical elements with the personal rule of D'Annunzio.

Ledeen argues that D'Annunzio's genius as a political leader stemmed from his understanding of the importance of style in mass politics. D'Annunzio, rather than Mussolini, invented the balcony discourses and rhetorical gestures designed to win the attention of national and international opinion. Yet, despite the title, the author denies any intrinsic connection between D'Annunzio's politics and Fascism. (He does not, however, deny the substantial similarities between D'Annunzio's following and the Fascist movement before it ossified into a regime.) But by separating the poet's political style from the substance of the Fiumian question as an issue in Italian politics and by emphasizing the radical elements attracted to D'Annunzio's banner, Ledeen risks some distortion. In Fiume D'Annunzio became a pawn in a larger political game. Because he substituted style for deeper political convictions, the poet could never define the content of his gestures. Others, however, were more than happy to do so. D'Annunzio's rule was costly theater which paved the way for Fascism by concrete example and by undermining respect for Italy's political institutions. Despite these reservations on the way, Ledeen fits Fiume into the larger picture, this book is a challenging and important reconstruction of one of the strangest incidents of the century.

ALEXANDER DE GRAND
Roosevelt University

GEORGE W. BAER. *Test Case: Italy, Ethiopia, and the League of Nations*. Stanford, Calif.: Hoover Institution Press, 1976. Pp. xiv, 367. $14.95.

This impressive and important book, a sequel to the author's *The Coming of the Italian-Ethiopian War*, should remain for many years a standard account of Europe's feeble and half-hearted response to Mussolini's aggression in Ethiopia. The author has done extensive research in published and unpublished documents, including the British Public Record Office, the *Archivio Centrale dello Stato* in Rome, the Mussolini Papers, and Department of State documents in the National Archives. He has not consulted the files in the *Archivio Storico, Ministero degli Affari Esteri*, but has uncovered a wealth of material about the Italian position from other sources. The themes in this book are not entirely new, but never before have they been documented in such a thorough and authoritative manner. Mussolini was out to get all he could of Ethiopia short of war in Europe. He used threats to intimidate Britain and France and air power and poison gas to crush Ethiopia. British and French officials cared little for the fate of Ethiopia, but the British were under strong pressure from public opinion to support the League. The British-French game was to exert limited pressure on Mussolini by economic sanctions which would satisfy public opinion and make him more willing to negotiate a settlement.

Serious friction developed between France and Britain over the degree of pressure to exert on Italy. France was opposed to an oil embargo, as eventually was Russia (which actually increased its exports to Italy after sanctions were applied). The British were more favorable to the oil sanction and were certain that they could defeat Italy in war. Many factors, however, demanded that Mussolini not be pressed too hard. Mussolini's defeat might bring a Communist Italy. The Far East demanded attention. Too much pressure might push Italy into the German camp. Britain was not immediately ready for war and was never quite certain of French support. The Hoare-Laval conversations in December 1935, which led to the proposal to give Italy a large part of Ethiopia, were primarily concerned, according to Baer, with easing tensions between France and Britain. The closure of the Suez Canal, the one sure way to hurt Italy, was apparently never seriously considered.

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Limited sanctions might have worked if early predictions of a war of two or more years had been correct. Ethiopia received little help from any side. Germany gave limited support to Mussolini except for coal sales, and gave small amounts of material help to Ethiopia in order to prolong the war. The war gave Hitler his opportunity to remilitarize the Rhineland. Sanctions failed to make Mussolini more conciliatory and increased his popularity in Italy. Ethiopian resistance collapsed in seven months.

WILLIAM C. ASKEW
Colgate University

KVETA KUČEROVÁ. *Chorváti a Srbi v Strednej Európe: K etnickým, hospodárskym a sociálnym otázkam v 16–17 storočí* [The Croats and Serbs in Central Europe: About Ethnic, Economic and Social Questions in the Sixteenth and Seventeenth Centuries]. Bratislava: Vydavatel'stvo Slovenskej Akadémie Vied. 1976. Pp. 327. Kčs 47.

Current tension between Yugoslavia and Austria over Vienna's non-fulfillment of clauses in Article Seven of the 1955 State Treaty, designed to secure the existence of Croat- and Slovene-speaking minorities in the provinces of Burgenland and Carinthia, lends a particular topicality to Kveta Kučerová's study. In this account of the diaspora of the Serbs and Croats caused by the Ottoman Turkish advance beginning in the latter fourteenth century, the Croats receive the bulk of attention. (The historically more autochthonous Slovenes are mentioned only marginally). Never before has the great migration from the central Balkans into present-day Hungary, Italy, Romania, western Austria, Moravia, and Slovakia—largely a sixteenth-century phenomenon in the case of the Croats—received such thorough treatment. The book skilfully combines social, economic, military, and political strands on the basis of an exacting approach to the various available sources. The author's industry in collecting materials is formidable: she has not only utilized the copious holdings of the Slovak State Archives and records in Budapest but has also obtained photocopies of documents from Belgrade. The plethora of more esoteric languages required for this kind of research has been no obstacle for her. Unfortunately, she was not allowed to go to Vienna, only thirty-five miles from her home, and so, she says, she had to rely solely upon extant literature (p. 43). It follows that Kučerová could not collect first-hand impressions in next-door Burgenland, the principal location of the remaining Croatian ethnic islands. In all events, her judgment is uniformly cautious and sound. She notes, for example, that (modern) "statistical data are a manifestation of subjective national consciousness and partly a product of national state policy" (p. 32).

Since an adequate summary of the volume's contents is impossible here, I refer the reader to a reasonably accurate German résumé on pages 300–05 (or one in Russian, pp. 295–99). Suffice it to say that the writer distinguishes between the mainly military nature of the Serbian exodus—Hungary's and Austria's need for more soldiers—and the chiefly agricultural character of the Croatian migration. In the latter instance the refugees were welcome because of the need for dependent peasant labor, and in fact the new hands made a great contribution to overall economic progress. Kucerová duly emphasizes the role of the nobility as the dominant social stratum, and gives special treatment to Slovakia's "Burgenland" (*Gradišče*) Croats. Examination of the difficult problem of family names is made possible by the survival of several Croat-speaking villages in the vicinity of Bratislava.

There are few faults in this book. The reviewer does notice the absence of titles in German and English on the Croatian military frontier (e.g., the work of Gunther Rothenberg). Maps are missing entirely, although the author refers to her use of them and Bratislava itself is a reknowned center of cartographic production.

THOMAS M. BARKER
State University of New York, Albany

NICOLAE STOICESCU. *Vlad Ţepeş* [Vlad the Impaler]. Bucharest: Editura Academiei Republicii Socialiste România. 1976. Pp. 238. 20 L.

This work reflects the concern of Romanian historians to express their viewpoint in the "Dracula Debate" which has attended the explosion of public interest in the subject, following the publication of Raymond T. McNally and Radu R. Florescu's *In Search of Dracula* (1972). (A symposium on this subject was recently held at Ohio State University under the sponsorship of the American Association of South East European Studies involving scholars from Romania and the U.S.).

The ostensible occasion for the publication of Nicolae Stoicescu's book (as well as Stefan Andreescu's *Vlad Ţepeş [Dracula] Between Legend and Historical Truth* and Constantin C. Giurescu's *The Life and Deeds of Vlad Ţepeş Dracula)* is the 500th anniversary of the Impaler's death (December-January 1976–77); the real cause is the desire of Romanian historians to correct certain misconceptions concerning Vlad Dracula's reputation for cruelty and to emphasize his heroic and na-

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tional stature. It is interesting to observe that apart from a few specialized articles on specific segments of his career and a handful of monographs on the German and Russian narratives concerning Vlad, no exhaustive study has been published in Romania since Ioan Bogdan's controversial monograph, *Vlad Ţepeş and the German and Russian Narrations Concerning Him* (1896), which in effect established his reputation as a villain.

Stoicescu takes issue on a number of minor points with Florescu and McNally's earlier scholarly work—the first exhaustive study on Vlad the Impaler in any language, (*Dracula: A Biography of Vlad the Impaler, 1431–1476.*) In essence, however, in terms of the primary and secondary sources consulted, and using the same methodology and similar chapter headings, his chronological divisions and main conclusions are closely identical; and Stoicescu frequently cites the earlier work in both his text and footnotes.

Stoicescu's work is divided into four parts, three of which deal with historical events: Dracula's first rule and exile (1448–56); his domestic and foreign policies during his principal reign (1456–62); the period of imprisonment and final rule (1462–76). Part four treats the propagation of the Dracula myth both abroad and in Romanian literature.

By far the best chapter involves Dracula's campaign against Mohammed the Conqueror in 1462, for which primary sources (Romanian, Turkish, and Byzantine) are ample, and we can follow the progress of the war from both sides of the battlefront. It is difficult, however, to accept Stoicescu's labelling Vlad "victorious" since he lost the throne as a consequence of the Turkish action, even though Wallachia survived as an autonomous state.

Vlad Ţepeş brings a number of new documents to light, is exhaustively researched, has an impeccable chronology (a problem in fifteenth-century Romanian history), clarifies a number of controversial points, and rejects some erroneous interpretations. For instance, Stoicescu argues, with a good deal of logic, that since Dracula spent four years at the Turkish court as a hostage of the Sultan, it is inconceivable that he would have been unaware of the fact that Muslim custom prescribed wearing turbans even in the presence of important rulers. He concludes, therefore, that the episode involving Vlad's alleged order to have the turbans (or fezzes) nailed to the heads of the Turkish ambassadors in all likelihood involved hats worn by a non-Turkish delegation in his presence, clearly offensive to his sense of protocol.

Those readers who expect Stoicescu's book to be a biography of Vlad the Impaler will be most disappointed, and it is conceivable that in the absence of new documentation a definitive biography cannot be written. Stoicescu seems to give up the attempt from the very start; he has little to say about Vlad's birth, his youth and education, the four-year period of Turkish captivity, his residence with János Hunyady, the twelve-year (?) Hungarian captivity, or his marriage and family life. There is no attempt at a character analysis or physical description, and not enough on his political and religious ideology. Instead of focusing on Vlad the Impaler during the interregnum periods, the study centers on obscure princes who ruled in the interval: Vladislav II (1448–56), Radu the Handsome (1462–75), and Basarab Laiota (1473–75). This makes for an excellent monograph on late fifteenth-century Wallachia, with Vlad the Impaler little more than a pretext for a title.

RADU R. FLORESCU
Boston College

JOSEF KORBEL. *Twentieth-Century Czechoslovakia: The Meanings of Its History*. New York: Columbia University Press, 1977. Pp. xii, 346. $14.95.

The subtitle of this study suggests its aim better, perhaps, than does its title. The most important part of this book is not a survey of Czechoslovakia's twentieth-century history but rather a psychological profile of its leading people—the Czechs. The Slovaks are dealt with here as a Czech appendage, that is, not as a separate people with their own history, ideas, and aspirations but only as they relate to Czech development.

In endeavoring to explain the modern Czech mind, the late Josef Korbel—who was a political scientist and Czechoslovak diplomat—ranges widely over their history. Westernmost of the Slavs, the Czechs were brought into the sphere of the Western (Roman Catholic) Church in the ninth century, thus assuring their Western cultural orientation. The medieval kingdom of Bohemia, associated with the Holy Roman Empire from the tenth to the nineteenth centuries, was organized along West European feudal lines. In the fifteenth century it became the center of the Hussite religious movement, which saw the Czechs defy all of Catholic Europe and prepared the ground for the Protestant Reformation in the following century. In the seventeenth century Bohemia became the focal point of the Protestant revolt against the Catholic Habsburg dynasty of the Holy Roman Empire. Although the Habsburgs failed to save the Catholic cause in the whole Empire, they were entirely successful in Bohemia. They deprived it of its sovereignty, merged it with Austria into a common Austro-Bohemian state, and forcibly recatholicized its population. From the viewpoint of modern Czech nationalism,

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what was worse than their recatholicization was their reduction in social status. The expulsion of the native Bohemian Protestant nobility and its replacement with an alien Catholic one, together with the germanization of the Bohemian towns, reduced the Czechs to a nation of peasants until the nineteenth century. Then, under the influence partly of the ideas of the French Revolution and partly of Romantic German nationalism, they effected a national revival.

Historiography played an important role in rekindling Czech national consciousness. Most Czech historians and thinkers accepted the Hegelian view that a nation's history was not an accidental series of events but a meaningful unfolding of an idea, and diligently searched Czech history for its "meanings." Korbel summarizes their views, from Palacký through Masaryk to contemporary writers. Whether their writings on the meaning of Czech history have validity or not, they have helped to shape the modern Czech mind, which is characterized by a secular and rational outlook and a belief in the virtues of egalitarian democracy and social justice. The establishment of the Czechoslovak Republic under Masaryk's presidency in 1918 was the realization of a century of Czech dreams and strivings. Korbel gives much attention to the Czechs' repeated failures to maintain their country's independence and democracy. Unlike most Czech exiles who tend to blame other nations for this, he is willing for the Czechs to accept part of the responsibility. "In 1918, Czechoslovakia gained independence without firing a shot; twenty years later, the nation lost it without firing a shot." Although Korbel's study of Czechoslovakia does not present much new information, it is a thoughtful and stimulating one and deserves serious attention.

VICTOR S. MAMATEY
University of Georgia

KAZIMIERZ POPIOŁEK. *Śląskie dzieje* [Silesian History]. Cracow: PWN, 1976. Pp. 535. 80 Zł.

This is a curious book. We cannot speak of its scholarly merits, for it has none; it is an attempt at popularizing the history of Silesia for gullible readers. Kazimierz Popiołek, a good Marxian nationalist, has no difficulty in fitting the history of Silesia into the Hegelian scheme. Upon the thesis of the medieval history of Silesia within the limits of the Polish state followed the antithesis of the successive rule of the Luxemburgs, the Habsburgs, and the Hohenzollerns. It was under the rule of Prussia and Germany that the fortunes of Silesia sank to their lowest level (a very peculiar interpretation from the Marxist point of view, for it was precisely the period of a dynamic development of the "forces of production" in Silesia). The synthesis was enacted in two stages: after World War I part of Upper Silesia returned to Poland, but the final fulfillment of all the efforts and sacrifices ever made by the Silesian people was achieved only in 1945 when the whole of Silesia became an integral part of the Polish People's Republic, and its inhabitants were united with the rest of the nation (p. 509). This event restored to Silesian history its full meaning and sense.

It is a travesty to speak in this way of the fate of the Silesian people. In 1945 the Polish population formed a substantial portion of the whole population only in Upper Silesia; Lower Silesia was thoroughly German. It is an absurdity to speak of the liberation of Lower Silesia and of the unification of its inhabitants with the Polish nation, unless by liberation we understand the annexation of the territory, and by unification the expulsion of several millions of people. Silesia did not become part of Poland because of its historical past, but as a result of Soviet conquest and Stalin's decision.

If the incorporation of Silesia into the Polish People's Republic constituted the ultimate stage of history, one would expect that the Communists did everything they could to bring this about. What a disappointment! Popiołek dug out an obscure leaflet of the Communist Party of Poland from 1937 complaining of the policy of germanization in the Opole region (p. 435), and this is all he could produce. In this one instance we can safely trust him. The fact is that the Communist Party of Poland did not want to know anything of the independence of Poland, not to speak of such a trifle as Polish rights to Silesia.

When history is treated as ancillary to politics and pedestrian historiosophy, suppression and distortion of facts become the virtues of a historian.

ANNA KAMINSKA
University of Oregon

DANIEL STONE. *Polish Politics and National Reform, 1775–1788*. (East European Monographs, number 22.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, New York. 1976. Pp. 122. $10.50.

This is a penetrating analysis of the dramatic period in Polish history between the First and Second Partitions. It supplements *The First Partition of Poland*, by Herbert Kaplan, and *The Second Partition of Poland*, by Robert H. Lord.

Stages in the unfolding picture of those thirteen grave years include description of "the movement for national reform," assessment of the Permanent

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Council in the framework of diverse political forces, and a fresh articulation of political parties and platforms. Of particular value for an English-reading student of Polish and Russian history is an excellent description of Russian penetration of Poland, putting the role of St. Petersburg's ambassadors to Warsaw in relief. "Epilogue: Reform and Partition" as well as "Conclusions" would probably justify extension of the subtitle to 1775–94.

This study, both instructive and enjoyable, displays fine scholarship and the author's concern. But, it also arouses some questions and reservations. The key to and explanation of the picturesque democracy of the Polish gentry appears in the study to be the Sarmatian character of Polish nobility; this does not say much if anything. One would prefer more objective explanation related to the type of ownership that had developed in Poland already in the fifteenth century, that was equal to the German "*kaiserliche Ritterseigentumschaft*," and that was acquired in Poland not only by a few families but by eight to ten percent of the nation. That type of sovereign ownership also was spread to Lithuania and other parts of the Commonwealth.

The reforms of the Great Diet need broader and more convincing analysis of the previous transformation of the whole feudal society into a more modern society. Absolutism was an instrument of progress in some areas and times; Poland needed other instruments for progress. Lack of a sufficiently strong and aggressive executive branch of government explains the failure of Poland to face responsibly the growing centralization and imperialistic militarism of its neighbors. When the Polish-Lithuanian Commonwealth limited its armed forces to 26,000 in 1717, it faced a 330,000-strong Russian army that soon increased to 1,000,000 and an 83,000-man Prussian army that soon reached 250,000. Poland never caught up with those who quickly became aggressors and partitioning powers.

Broader issues not sufficiently presented include extension of privileges—"*Neminem captivabimus*" and "*Nec bona recipiantur*" (habeas corpus rights in force in Poland from 1425–35) to all inhabitants of cities in the 3rd May 1791 constitution, and to all Jews by administrative ordinance in 1792. Some differentiation between "serfs" in Russia and Poland would help in the better understanding of both. Some small mistakes could be eliminated, as for example, the Magdeburg and "Chelm municipal" law should be Chełmno (and Środa should be added). Admirable success in the proper spelling of hundreds of Polish names would be perfect if galley proofreading had not failed on some of them: for example, the Połaniec Manifesto was issued in Połaniec and not Polanice. Such small imperfections do not decrease the obvious value of this study.

WACŁAW W. SOROKA
University of Wisconsin—Stevens Point

HENRYK ŻALIŃSKI. *Kształt polityczny Polski w ideologii Towarzystwa Demokratycznego Polskiego (1832–1846)* [The Political Shape of Poland in the Ideology of the Polish Democratic Society (1832–1846)]. (Prace Komisji Nauk Historycznych, number 36.) Summary in French. Wrocław: Ossolineum, 1976. Pp. 166. 40 Zł.

Following the suppression of the November Uprising (1830) by the Russian army, thousands of Poles became part of the "Great Emigration" that found political asylum in France. There they had the freedom to form political organizations and to express their views freely both in speech and in the press. As is so common among political exiles, the Poles disagreed vehemently on how to regain independence and what should be the nature of their restored homeland. The Polish Democratic Society, which represented the largest number of emigrés, engaged its membership in lengthy, thorough discussions of these problems. These discussions and resulting decisions comprise the contents of Henryk Żaliński's book.

What strikes the reader immediately and most forcefully is the radicalism of the Society's views. Oddly enough, these were largely opinions of Polish gentry, but the program was designed to benefit the Polish "people" (*lud*) without whose support independence would not be regained. Reborn Poland would be completely restructured—socially, economically, and politically. Equality was a key word in their program: the land would belong principally to those who tilled it, and sovereignty would reside in the people. Political power would be expressed through a democratic republic whose president and single-chamber legislature would be elected *directly* by the people. Besides the familiar three branches of government—executive, legislative, and judicial—the Society envisaged an elected supervisory branch (*instytucja zachowawcza*) to see that the other branches carried out their functions properly.

The restored Poland would be within the frontiers of 1772, although some of the Society members even talked of a Poland from the Baltic to the Black Sea and from the Oder to the Dnieper. The non-Polish elements in a restored Poland would present no problem, for there would be complete equality so far as race, religion, and ethnicity were concerned.

The creative minds of some of the Society mem-

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bers, familiarity with such views as those expressed in the *Declaration of the Rights of Man and Citizen* and other writings, knowledge of existing constitutions (including the American), and the romantic nationalism of the times—these are some of the factors that explain the Society's radicalism. But with the failure of the revolutions of 1846, 1848, and 1863, the Society's members never had an opportunity to implement their ideas.

The author begins his work with a bibliographical essay that tells of the inadequacies of previous studies of the subject and concludes it with a French summary. Readers unfamiliar with Polish will find a more useful and more detailed summary of the Polish Democratic Society's political program in the long article by Peter Brock that appeared in the spring and summer 1969 issues of the *Polish Review*.

CHARLES MORLEY
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MIRA LIEHM and ANTONÍN J. LIEHM. *The Most Important Art: East European Film after 1945*. Berkeley and Los Angeles: University of California Press. 1977. Pp. 467. $23.50.

In the West, literature dealing with the cinema of Eastern Europe and even the Soviet Union in the postwar period is almost nonexistent; one encounters little apart from the occasional journal article reviewing the comparatively few films shown at international festivals. The most significant contribution of the book by Mira and Antonín J. Liehm is that it fills this great lacuna in the history of world cinema for the English-speaking reader, tracing the development of film production over the last thirty years in the European Communist countries and the USSR.

It was during this period that events of great consequence for these nations took place: the seizure of power by Communist parties in Eastern Europe and the resultant changes in political and cultural life; the death of Stalin and the consequent relaxation of strict governmental control; and, finally, since the second half of the sixties, the return to an almost Stalinist measure of rigidity. The cinema, the most sensitive indicator of the pulse of social life, reflected these transitions in many varied ways, proving to be indeed "the most important art." It was with these words—which the authors employ as the title of their book—that Lenin, on the whole largely indifferent to art, defined his attitude to the phenomenon of cinema. This designation, often cited in the official criticism, has acquired double meaning in the socialist countries. On the one hand, the cinema became the most powerful and widely employed means for the dissemination of political propaganda and the officially sponsored moral values. On the other, because of the very nature of cinematic language, it was the only possible medium for an artist to protest against the norms established for life in these societies. This dual social function of cinema is the main theme of the work which the authors explore after a brief but succinct chapter on the appearance and development of film in Eastern Europe and Russia.

This short introduction provides the reader with the background necessary for an understanding of the events in the world of film-making during the last three decades. Apart from the degrading influence of the censorship and ever-demanding Socialist Realism in all spheres of art, the artistic quality of the films released toward the end of the forties was determined to a considerable extent by the varying levels of technological development and cultural traditions, as well as the previously differing political systems in each of these countries. In discussing the pressure of the censorship, however, the authors frequently refer to the "teachings of Zhdanov" as being fundamental to the establishment of norms in art and literature acceptable to the government. While Zhdanov undeniably played an exceedingly odious role in the cultural life of the Soviet Union and the other socialist countries, "teachings" perhaps ought not to appear without quotation marks: the militant tirades of the party boss, for whom sheer abuse was the only acceptable method of polemics, can hardly be termed teachings.

The Western reader will be surprised at the role of governmental policies, not only in the organization and running of the studios, the production and release of films, but also in the choice of subject matter and style, all of which the authors discuss with deep knowledge and understanding. Occasionally, however, their account reduces to a simple listing of directors and films, which probably says little to a reader unfamiliar with the great majority of these films. The best chapters in the book deal with the cinema of Czechoslovakia, which the Liehms left in 1969. The selected bibliography and the large number of stills are additional features of a book containing much of interest and value, not only for specialists but also a wider reading public.

DODONA KIZIRIA-SMITH
Indiana University, Bloomington

BASIL DMYTRYSHYN. *A History of Russia*. Englewood Cliffs, N.J.: Prentice-Hall. 1977. Pp. xvii, 645. $14.95.

In some six hundred tightly printed pages Basil Dmytryshyn has channeled the millenial flow of

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Russian history through the familiar conduits of "Kievan Rus," "Divided Rus," "Muscovy," "Imperial Russia," and "Soviet Russia." (The final section is based on the author's *USSR: A Concise History*.) The book is well equipped with the apparatus of reader assistance: lists of suggested readings, charts delineating the succession of rulers, administrative diagrams, eight maps, a variety of half-page illustrations, and an index. Focused primarily on political history (domestic developments and foreign policy), it provides lighter coverage in social, economic, and cultural areas.

The work can be commended as a relatively compact and reasonably comprehensive survey. But there are chinks in the impressive armament and some problems behind it. An opening attempt to introduce the reader to Russian source materials and historiography is laudable, but the forest is lost in a thicket of specific references. And the problem recurs. The mark of Isaiah Berlin's fox stamps the text: details tend to obscure designs. Reading lists are uneven, flawed by dubious entries and surprising omissions. There is excessive use of Russian terminology (and some pluralization that can be described only as highly singular [*aktys*, *tiaglye dvors*]); maps are poorly distributed; and some material is repetitious. The occasional factual slips spotted are extenuated in most cases by the presence of more accurate information in other locations. But more numerous than explicit misstatements, and therefore more worrisome, are instances where imprecision in the text may create an erroneous impression. Does the author really mean to suggest that all serfs were in *contractual* bondage (p. 220)? Or that the peasants' private plots (the old *usad'by*) originated at the time of collectivization (p. 537)? Here and elsewhere the reader may be confused or misled.

Such problems aside, the work contains a substantial amount of information; and if a concentration of particulars makes for slow reading at times, some may find appeal in descriptions of rulers where bright virtues are neatly balanced against somber (but deucedly more interesting) vices. Unfortunately, however, such a presentation is ill-calculated to inspire thoughtful consideration of the complex effects of power on personality. And the treatment of tsars and tsarinas is merely symptomatic of the general lack of an analytical dimension in the work.

Yet in fairness it should be recognized that the book has been presented only as a summary which "can in no way pretend to be either a definitive or an exhaustive work" (p. xv). Those who simply "want the facts" may find it useful. One could do worse. But those who turn to history for deeper understanding of the range of human experience, for insight into society and self, are less apt to be content with it. In the available literature, one could do better.

DOROTHY ATKINSON
Stanford University

A. D. LIUBLINSKAIA *et al.*, editors. *Problemy paleografii i kodikologii v SSSR* [Problems of Paleography and Codicology in the USSR]. Moscow: Izdatel'stvo "Nauka," 1974. Pp. 441. 2 r. 62 k.

This symposium contains contributions by leading Soviet experts on medieval Slavic, Greek, Latin, Armenian, and Georgian manuscript studies and epigraphy and is an excellent indicator of the considerable recent achievements of Soviet scholarship in those fields. While many of the articles are appropriately technical, others are primarily bibliographical surveys, ranging from L. V. Cherepnin's examination of Russian scholars' attempts at defining the subject of paleography to a series of articles on Armenian and Georgian manuscript study, which are useful for general information but have little to offer the specialist.

Soviet experts in Latin and Greek paleography are few, but their work is of the highest quality, most being students respectively of the volume's responsible editor, A. D. Liublinskaia, and one of the contributors, E. E. Granstrem. The Latinists are fully conversant with the new techniques developed by Western medievalists, as one can see in the recent book by L. I. Kiseleva on Gothic cursive in the thirteenth to fifteenth centuries and in the work of V. L. Romanova, whose contribution here is a portion of her splendid new monograph on the manuscript book and Gothic script in thirteenth- and fourteenth-century France. Also of particular interest for Western medievalists is T. P. Voronova's study of some little-known fourteenth-century financial documents from Normandy in the Leningrad Public Library.

Two of the contributions on Greek materials merit special attention. V. L. Fonkich provides the first article of a series in which he will analyze the numerous sixteenth- and seventeenth-century Greek documents in Soviet collections (here he covers patriarchal charters of the second half of the sixteenth century). In a continuation of her earlier work, with which she assumes the reader is familiar, Z. G. Samodurova surveys and classifies according to content all the manuscripts containing short Byzantine chronicles. Somewhat surprisingly in a volume covering codicology, her article suffers from an apparent insensitivity to the need for detailed codicological analysis of each manuscript if the analysis of its content is to be related to a meaningful chronological framework.

As a student of Muscovite history, I find particularly valuable the contributions by N. V. Sini-

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tsyna, B. M. Kloss, and L. M. Kostiukhina, all of whom have been working on the previously little-studied problem of identifying schools of copyists and hands of individual Muscovite scribes. After being so disgracefully served by the publisher of her important monograph on seventeenth-century Muscovite semi-uncial hands, Kostiukhina should be pleased to see a portion of that same study appear in this comparatively well-produced volume. The material in her article can scarcely be appreciated and used, however, in isolation from the rest of her monograph. Both her article and that of Kloss on the Muscovite Metropolitan's library in the sixteenth century could have used even more and better illustrations. While these and the other Slavists whose contributions fill nearly half the volume are making solid progress, clearly their techniques are not as sophisticated as those used by their Latinist colleagues. S. L. Kashtanov, the only Slavist in the volume who seems equally at home in the literature on Latin paleography, might well be heeded where he points out in his perceptive review article on Gasparri's *L'écriture des actes de Louis VI, VII et Philippe Auguste* the relevance of that author's methodology for those studying Russian paleography and diplomatics (p. 318).

While one hopes that this symposium will help to broaden horizons of scholars who read Russian, it should and could have served a similar function for medievalists who do not read Russian had the articles been provided with at least summaries in other scholarly languages.

DANIEL CLARKE WAUGH
University of Washington

R. E. F. SMITH. *Peasant Farming in Muscovy*. New York: Cambridge University Press. 1977. Pp. xii, 289. $22.50.

In an earlier book, *The Origins of Farming in Russia* (1959), R. E. F. Smith presented a remarkable analysis of the evolution of agriculture in the Russian plain from Neolithic times to the Mongol conquest of the thirteenth century. The central theme of that work was the linkage that held together the changes in farming techniques, the evolution of farming implements, and the modifications in the structure of rural society. The present book continues the history of Russian farming, for as Smith points out, agriculture in Muscovy was predominantly peasant farming, organized in small household and mainly family units. In this volume, as in the earlier one, the central theme revolves around techniques, implements, and social structure. Like the earlier book, too, this study is illumined by its author's careful scholarship, his learning, his refusal to be daunted by the difficulties of his subject, and most important, by the significant contribution the book makes to the study of early Russian economic history.

The book begins with the Mongol invasion and goes to the mid-seventeenth century, though, as the author explains, the scantiness of the data compelled him to concentrate on the period from the fifteenth century. Smith deals principally with the independent peasant farmer, the "black" peasants as they were called, who lived in the forested zone of the Russian plain, rather than with the peasants on the estates of monasteries and noblemen.

In the troubled and uncertain times of the Mongol domination, peasant farms were thinly scattered through the forest zone. Land was so plentiful that there was no need for precise boundaries. Instead, the bounds were defined by the formula "wherever the *sokha*, scythe and axe have gone." The *sokha*, the primitive plow-like tool, represented the arable, the scythe the hayfield, and the axe the forest. Using the three elements of this formula as the organizing principle in the first section of his book, Smith discusses arable, hayfield, and forest, as well as the implements, in detail. He explains the contribution each made to the economy of the peasant household, taking usually justified exception on a number of occasions to the opinions of other scholars. He adds a fourth element, the household, which, as he rightly observes, was nearly always a nuclear family. When the children grew up they left to establish their own nuclear family homesteads in the abundant free land that stretched in every direction. "This," writes Smith, "was a major factor in that creeping colonisation which settled the east European forest zone" (p. 83). He concludes the first section with what he terms a simplified model, based on the exiguous data, of the production and consumption of the isolated peasant farmstead. The model omits, among other things, dues, taxes, nonagricultural income, and the "normal" size of a family holding. From his model the author draws the scarcely surprising conclusion that the peasant's arable, supplemented by hayfield and forest, produced enough to maintain his family and his livestock. (If that had not been the case, Smith would have had to choose another subject.)

The second section of the book presents a study of the peasant household unit in its relation to the world around it. To accomplish this, Smith decided to concentrate on three regions, all of them on a line at about the fifty-sixth parallel, but each with a quite different physical and cultural environment. Toropets in the west was on the Polish-Lithuanian border, the Moscow district lay in the middle of European Russia, and Kazan on the

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eastern frontier was basically a Tatar region. The chapters dealing with each of these regions and the concluding chapter spell out the impact of new pressures upon the social structure and upon the economy of the peasant farm. Of these, the most significant were the state-making activities of the rulers of Muscovy, their need to institutionalize and to regularize, and their demands upon their servitors for military service. In the emerging political and social climate, a more precise definition of property lines replaced the old formula of *sokha*, scythe, and axe, peasants no longer had a voice in the disposition of the land upon which they lived, and they themselves were in the process of being reduced to an estate labor force, and ultimately to serfdom. The author suggests that because of the isolation and the low-level economy of the household farm, the peasants could not cooperate effectively to resist the encroachments of the state. On the other hand, its simple technology and its relative self-sufficiency enabled the peasant farm household to survive the disasters that overwhelmed Muscovy in the last decades of the sixteenth and early years of the seventeenth centuries, and to recover from them with relative rapidity.

Smith has written a difficult book which demands slow and careful reading. The reader will then recognize that it is a model of the way in which thorough and exact research, an appreciation for the importance of technology and an understanding of it, combined with an imaginative methodology, can overcome the handicap of extremely thin source materials to create an instructive and convincing historical study.

JEROME BLUM
Princeton University

S. B. VESELOVSKII. *D'iaki i pod'iachie XV–XVII vv.* [Officials and Minor Officials in the Fifteenth through Seventeenth Centuries]. Moscow: Izdatel'stvo "Nauka," 1975. Pp. 606. 2 r. 94 k.

Stepan Borisovich Veselovskii compiled the reference work under review from manuscript and printed sources over many years, completing it in the 1940s. Like other works of the Stalin era by this great historian, it is only now being published. It is a list of Russian bureaucrats who served grand princes and tsars, *udel'* princes, governors [*namestniki*], and clerics in military and diplomatic affairs, in central judicial and administrative offices, as compilers of land inventories, and in local administrative and fiscal offices from the fifteenth through the seventeenth centuries. There are about 4,100 alphabetical entries in addition to cross references, listing each person, where possible, by surname, patronymic, first name, and nickname. Veselovskii supplies genealogical and other miscellaneous information and cites sources for much, but not all, of it. This catalogue complements Veselovskii's *Issledovaniia po istorii klassa sluzhilykh zemlevladel'tsev* [Investigations in the History of Serving Landowners] (1969) and his *Onomastikon* (1974), and fills a gap in reference works, its predecessors being smaller, out of date, or of narrower coverage. The entries range from minor *pod'iachie* and *d'iaki* [sub-clerks and clerks] to powerful *d'iaki* of the Council of Boiars who were chancellors of departments of state. The growth of this bureaucracy paralleled the emergence of autocratic institutions and it is not surprising that eighty percent of the entries are from the seventeenth century. The editors, V. I. Buganov and B. V. Levshin, are authorities in administrative history of early Russia, and it is unfortunate that they limited themselves to minor corrections and rearrangements of the text. Recent studies of new sources reveal further or corrective information about existing entries and names of many bureaucrats who are not listed (e.g., B. Plavsic's review, *Kritika*, 13:1, based on an investigation of the pay list in manuscript of the *Posol'skii prikaz* for 1646/47). The work, therefore, must be used with caution.

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Roosevelt University

G. S. GROSUL. *Dunaiskie kniazhestva v politike Rossii, 1774–1806* [The Danubian Principalities in Russian Politics, 1774–1806]. Kishinev: Izdatel'stvo "Shtiintsa," 1975. Pp. 219. 1 r. 41 k.

Even for those well acquainted with the extent and implications of centralization in the USSR, the qualitative gap between historical studies which appear in the center, i.e., Moscow and Leningrad, and those which are published on the periphery, in Kishinev, for example, is quite striking. What characterizes the latter is not a dearth of documentation, for such works are often well researched. But in matters of interpretation, of logical argumentation, they appear hopelessly doctrinaire even when compared to works published in Moscow. G. S. Grosul's study is a case in point.

The author's goals are two-fold: 1.) to establish the conditions under which Russian policy in the Danubian Principalities was formulated and carried out, and 2.) to describe the tactics used by Russian diplomats in their confrontations with Ottoman and Western representatives. Predictably, Grosul first attempts to establish a socioeconomic motive for Russian policy. After arguing at length that the desire of Russian landowners was to control the Black Sea grain trade, the author concludes with a non sequitur: "However, the main

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(factor) which directly influenced Tsarism's position . . . was the international position of Russia (and) the demands of international politics and strategy" (p. 37).

The core of the book consists of traditional diplomatic history. Relying heavily on the Foreign Policy Archives in Moscow, Grosul traces the fluctuations of Russian diplomacy between 1774 and 1806. There were two distinct phases in Russian policy. The first, characterized by Catherine II's "Greek Project," aimed at separating Moldavia and Wallachia from the Ottoman Empire, uniting them under Russia's aegis and thus creating a buffer state on Russia's southwestern border. Beginning with the reign of Paul I, Russia's policy entered the second phase. Because of the growing influence of England and France in the Middle East, Russia found it useful to maintain the integrity of the Ottoman Empire. However, in case the Ottomans joined France in a war against Russia, the latter designed a plan for the outright annexation of the principalities. Grosul feels that Russia's greatest advantage was her ability to interfere, almost at will, in the principalities under the guise of enforcing the terms of the Kücük Kaynarci Treaty. Her greatest diplomatic achievement during this time was the establishment of a consulate in the principalities.

Whenever the author attempts to generalize, however, as he often does, about the broader implications for Russia's policy, out pours a veritable litany of Soviet historical shibboleths. For example, the influence of revolutionary France in the principalities is presented as an extension of bourgeois capitalism while Russian policy "objectively" acts to revolutionize the masses and strengthens the traditional friendship of the Russian, Moldavian, and Wallachian peoples (this point is repeated at least twenty-five times throughout the book and reaches a crescendo of four repetitions on the final page). Even in Moscow such indulgence in ritualistic incantation would be considered overdone.

OREST SUBTELNY
Hamilton College

DONALD RAYFIELD. *The Dream of Lhasa: The Life of Nikolay Przhevalsky (1839–88): Explorer of Central Asia*. Athens: Ohio University Press. 1977. Pp. xii, 221. $13.50.

This slim but delightful volume is the first biography in English of one of Russia's greatest explorers, perhaps best known in the West for his discovery of the wild horse, *Equus przewalskii*. To my knowledge it is the only book-length English-language study of *any* Russian explorer and, as such, is a welcome and pioneering endeavor. The author, lecturer in Russian at the University of London, provides a wealth of detail regarding Central Asian peoples, geography, politics, flora, and fauna.

Przhevalskii led five expeditions—one into the newly acquired Ussuri region, the others into Chinese Inner Asia. Encouraged by Semenov Tian-Shanskii and Minister of War Miliutin, and driven by revulsion against urban society and by the irresistible attraction of the unknown wilderness, Przhevalskii became, in the author's words, "one of the first explorer-scientists" and at the same time "also one of the last explorer-conquistadors" (p. 210). His enormous contributions include the mapping of vast areas, collections of countless botanical and zoological specimens, and the writing of several books and diaries. A Russian expansionist, he advocated the forcible separation of Moslem and Buddhist regions from the Chinese Empire. Although he made several efforts to reach Lhasa, circumstances forced him each time to retire short of his long desired goal. (In all, only three Europeans were to get to Lhasa during the nineteenth century.)

The author presents a convincing and objective picture of Przhevalskii the man, with all his strengths, flaws, virtues, and quirks. An obstreperous and strong-willed individual, a misogynist, at times puritanical, a racist in his attitudes toward most Asians, especially the Chinese, Przhevalskii was also courageous and tender. Paradoxically, he believed in the superiority of European civilization over Asian, but himself scorned and avoided modern society.

The one serious flaw in this book is the absence of footnotes. Why? There is a bibliography, several photographs, and two maps.

KERMIT E. MCKENZIE
Emory University

MAUREEN PERRIE. *The Agrarian Policy of the Russian Socialist-Revolutionary Party from its Origins through the Revolution of 1905–1907*. New York: Cambridge University Press. 1977. Pp. xii, 216. $15.95.

The main loser among the political parties in the Russian Revolution, the Socialist Revolutionary Party has up to now fared little better in what scant attention it has received from scholars. The most substantial of the few works devoted to it within the past sixty years, two volumes by O. H. Radkey, are largely derisive toward the party and deal only briefly with its formative years. Focusing on this initial period, Maureen Perrie's book helps place the party's character in a much more balanced historical perspective.

The content of this study is considerably richer

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than its title suggests. Using the word "policy" in its broadest sense, the book actually recounts the entire relationship of the party with the peasantry. It shows that the chief issue in the agrarian policy was how to socialize the land with active support and approval of the masses. The party theorists, principally Victor Chernov, trusted not only to traditional attitudes they believed the repartitional commune had instilled in the peasantry, but also to a Marxian class analysis which defined the peasant small producer as part of the worker class, and therefore inclined toward a socialist consciousness. But when the party's official program became articulated it reflected ambivalence regarding the socialist potential of the peasantry. Moreover, upon formal adoption of the program in 1906, previously latent factionalism wrecked party unity. Then government repression and other policies compounded this so that even before the World War the party experienced the disorganization and indecisiveness which characterized it in 1917.

Perrie concludes that the party's failure to gain mass peasant support during its early period indicates that it overestimated the peasantry's current political consciousness. However, noting the influence of the SR agrarian program on Duma proposals and the party's success in the 1917 Constituent Assembly elections, she observes that perhaps the party was more accurate in its appraisal of the peasants' ultimate socialist potential.

This work is a well-written, convincing interpretation, soundly based on published primary materials, and it contributes importantly to the literature on its subject. Since it made little use of archival sources, it invites other studies when more of such materials become accessible.

ALLISON BLAKELY
Howard University

W. H. ROOBOL. *Tsereteli—A Democrat in the Russian Revolution: A Political Biography*. Translated by PHILIP HYAMS and LYNNE RICHARDS. (International Institute of Social History, Studies in Social History, number 1.) The Hague: Martinus Nijhoff. 1976. Pp. xi, 273. f 80.

Between February and October 1917 power in Petrograd passed from one small group of revolutionary politicians to another in rapid succession, and while the names of the principal leaders, Alexander Kerensky and Vladimir Lenin, are familiar to most people, a third figure, Irakli Tsereteli, the most important political presence from March until the July Days, remains a shadow among those more brilliantly illuminated by historians. The son of a leading Georgian publicist, Tsereteli (1881–1959) began his political activity as a student protester in Moscow, soon turned to Marxism, and joined the Menshevik movement which by 1905 dominated the Georgian intelligentsia and working class. At age twenty-five he was elected the youngest member of the Second State Duma where he emerged as the leader of the Social Democratic faction. When Stolypin dissolved the Duma in 1907, Tsereteli was arrested, tried *in camera*, and exiled to Siberia where he remained until the Revolution.

Returning to Petrograd in March 1917, Tsereteli was the architect of the political platform of the moderate socialist leaders of the Soviet. In his formulation of "revolutionary defensism," Tsereteli proposed that the revolution itself be defended by the peoples of Russia while simultaneously a campaign be carried on to achieve a "democratic" peace "without annexations or contributions." As the Soviet's influence increased at the expense of the Provisional Government Tsereteli was drawn reluctantly into participating in a coalition government made up of representatives of the Soviet and the *tsentsovoe obshchestvo* (the propertied classes). He soon became the foremost defender of the coalition and promoter of a military offensive against the Germans. As the revolution intensified in the summer months Tsereteli's influence declined, and the coalition proved inadequate to contain the deepening social cleavage between workers and the rest of the population.

When the Bolsheviks took to the streets in October, Tsereteli's role in Russian affairs ended. He returned to his homeland and from 1919 acted as diplomatic representative for the independent social-democratic republic of Georgia until it was overrun by the Red Army in February 1921. He then spent nearly a decade as the representative of the exiled Georgian Menshevik party to the Socialist International until he was driven out of the party by the more nationalist followers of his longtime rival, Noi Zhordania. He capped his long career by writing two volumes of memoirs, which remain an indispensable source for historians of 1917.

W. H. Roobol, a Dutch historian who has made extensive use of archives in the Netherlands and the United States containing Tsereteli's letters, has written the first major biography of Tsereteli, a volume to be set beside similar studies of other Menshevik leaders—Baron's Plekhanov, Getzler's Martov, and Ascher's Aksel'rod. Roobol has done us all a service by presenting clearly the major developments of Tsereteli's life and in particular his contributions to the moderate socialist phase of the revolution. But like some other biographies of revolutionary activists this study suffers from too little attention to the context in which these men and women worked. Tsereteli as drawn here is an

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isolated figure standing against a barely-sketched background. There is little discussion of the Georgian setting, no mention at all of the massive peasant movement in Georgia in 1902–06, no examination of the extraordinary strength of the social democratic movement in Transcaucasia or explanation of why Menshevism triumphed in Georgia. Similarly, in the chapter on 1917 the social milieu in which masses of people fought the revolution is left out. Thus any real explanation of Tsereteli's failure which goes beyond his personal liabilities is rendered impossible, though at one point Roobol notes enigmatically that Tsereteli "was not sufficiently attuned to the sentiments that lived among the masses" (p. 132).

The portrait that emerges of Tsereteli is of a practical and realistic democrat, a man of courage dedicated to the liberation of Russia and of Georgia, but one who had no real desire for power (in contrast to Roobol's Lenin). But one may derive another view of Tsereteli from Roobol's account. Able at a crucial point in 1917 to create a program for the moderate Left, Tsereteli seems never to have adequately understood the intensity of the social struggles which rapidly rendered his policy of class collaboration utopian and opened opportunities for advocates of Soviet power, government by the lower classes alone, such as Lenin. His failure to appreciate the sources of Bolshevik support led to his continual underestimation of the staying power of Lenin's government. Unwilling to risk civil war Tsereteli and the moderate socialists were swept along by events which overwhelmed their fragile institutions and hopes for parliamentary democracy.

RONALD GRIGOR SUNY
Oberlin College

CHARLES BETTELHEIM. *Class Struggle in the USSR.* Volume 1, *First Period: 1917–1923.* Translated by BRIAN PEARCE. New York: Monthly Review Press. 1976. Pp. 567. $18.95.

Charles Bettelheim, Director of Studies of the École Practique des Hautes Études and a leading theorist of the revolutionary socialist movement, offers in this volume a radical Marxist reinterpretation of the early years of Bolshevik rule. Written in the aftermath of the Soviet invasion of Czechoslovakia in the belief that "those who claim to be Marxists" must explain as well as condemn such acts, Bettelheim sees this book as the first of a three-part study, with subsequent volumes to cover the period 1924–53 and 1953 to the present. It is a difficult, engrossing, thoroughly provocative book, at times analytically brilliant, at times maddeningly overladen with an almost impenetrable jargon. It deserves, however, careful reading. Bettelheim raises challenging questions to Marxists and non-Marxists alike and forces attention on seminal issues, even if some basic answers seem, to me at least, wrong.

A brief review can only highlight the crux of Bettelheim's argument, which turns on a rejection of what he calls the "economistic problematic" as an explanation of historical development. Bettelheim means by this "the theses of congealed Marxism," which maintain that exploiting classes cease to exist in a proletarian dictatorship and that productive or economic forces are "the driving force of history" (p. 20). On the contrary, Bettelheim argues, the driving force of history is class struggle; and the dominant feature of early Soviet history is the defeat of the proletariat by bourgeois forces in the aftermath of revolution and civil war. Rather than constructing socialism, the Soviet Union consequently developed into "a capitalist state of a particular type" (p. 46), a phenomenon which has disoriented revolutionaries, ideologically disarmed Soviet workers, and drastically weakened the international socialist movement by falsely identifying the Soviet system with socialism.

How did the bourgeoisie triumph in Russia, and what might have prevented this? Bettelheim decisively rejects the argument that the party itself was a "demiurge," responsible for all successes and failures, and looks instead at the nature of class relations and the class struggle. For various reasons, he argues, some related to the exigencies of war, some to Russia's economic emergency, objective bourgeois social relations could not be destroyed or abolished, despite the expropriation of Russia's private bourgeoisie. Severe wage differentials and other inequalities remained, as well as command economic management, with special managerial privileges for capitalists and bourgeois specialists. In the countryside, the expropriation of Russia's gentry was followed only by a consolidation of the petty bourgeois middle peasantry, whose status and power was greatly enhanced by the NEP. In the process, a burgeoning state administrative apparatus merged with, and then overpowered, the party itself, replacing revolutionary commitment with managerial refusal to submit to rank-and-file control. This bourgeois victory, moreover, was the "result of objective social process, the outcome of class struggle, not the 'product' of theoretical or organizational conceptions of the Bolshevik party" (p. 251).

Bettelheim believes this process might have been prevented had the Bolshevik leadership not lost sight of the fact that the "masses are the determining factor in all historical transformation," ignoring in the process their ideological heritage (p.

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345). In a sense he argues that October was a "defeat" for the Bolsheviks, since it removed the necessity of party-mass identification: the fish-water relationship on which Chinese socialism was successfully built. NEP might have allowed the party to re-establish a mass base, Bettelheim suggests, but Lenin's illness left him powerless against an entrenched state apparatus, and his own "mass line" arguments went unheeded.

What is the difficulty in this analysis? Surely not the notion that civil war involved a strengthening of state control, or that social relations which one can reasonably label "bourgeois" re-emerged after 1917 to smother proletarian aspirations. The "mass line" is also an important concept in understanding the outcome of Russia's revolution and civil war, and one can look forward to Bettelheim's treatment of Stalin's cultural revolution at the end of NEP. Rather, the problem lies with Bettelheim's interpretation of both the party's and Lenin's role in the process of revolutionary change, an interpretation which sees Lenin himself as a strong advocate, like Mao, of a "revolutionary mass line" and a "swimmer against the tide" (p. 414), and the party as historically blameless for Russia's failure, but still capable of having averted disaster by recognizing its errors and mobilizing workers and poor peasants against the apparatus. In several extended sections crucial to his argument, Bettelheim insists that Lenin consistently stressed workers' initiative as a source of instruction for the party, maintained confidence in the revolutionary energy of the proletariat, and went so far as to call for a merger of the party and advanced proletarian elements: "the mass line in new forms." Lenin's writings during the first months of NEP, as well as his later writings in 1922 and 1923, "urgently stressed the need vigorously to apply a mass line once again," as a means for achieving the revolutionary transformation of economic relations (p. 493).

This is a wishful reading of Lenin, particularly of his last major essay, "Better Less but Better." Lenin's concern here and elsewhere is not for a mass line, but for a streamlined, more effective party apparatus, one implicitly more elitist. Lenin never trusted control from below, least of all in the countryside; and his enormous anxiety in the early twenties had overwhelmingly to do with the question of party stability and Stalin's heavy handling of the explosive nationality question, not with turning the party in a Maoist direction. As for the party itself, Bettelheim's argument seems to me excessively volunteerist. If a revolutionary political line could have prevented the growing dominance of bourgeois social relations, the Bolsheviks' weakness in this regard hardly seems "blameless." On the other hand, if the decimation of radical workers in one of history's bloodiest civil wars fundamentally weakened the party's revolutionary commitment, and if the overwhelming press for survival in catastrophic economic circumstances impelled directiveness, as I think occurred, then the power of economic circumstances in structuring class relations cannot be diminished, at least not as completely as Bettelheim insists.

These and other points need amplification, impossible in this brief review. And argument aside, the great importance of Bettelheim's book is his shrewd focus on the essential questions, questions one hopes will continue to be debated, and from which a clearer picture of the determinant period in modern Russian history may emerge.

WILLIAM G. ROSENBERG
University of Michigan, Ann Arbor

R. SH. GANELIN. *Sovetsko-Amerikanskie otnosheniia v kontse 1917—nachale 1918 g.* [Soviet-American Relations at the End of 1917—Beginning of 1918]. Leningrad: Izdatel'stvo "Nauka," Leningradskoe Otdelenie, 1975. Pp. 201. 68 k.

This study is a continuation of R. Sh. Ganelin's earlier work, *Rossiia i SShA, 1914–1917: Ocherki istorii russko-amerikanskikh otnoshenii* (for the review of this book, see *AHR*, 78 [1973]: 1018). Here, however, he examines a period already studied extensively by Soviet historians. Saying that the documentation for this period requires "special and systematic study . . . observing the demands of scientific criticism" (p. 3) he presents, what is for Soviet historiography, the novel thesis "that the unfortunate turn, taken in the relations between the young Soviet state and the United States, was not inevitable" (p. 4). Although Wilson and the State Department pursued a policy of unrelenting hostility to Soviet Russia, a small band of "political realists" sought to promote cooperation based on the pursuit of mutual economic advantage. Raymond Robins, William Boyce Thompson, Thomas Lamont, and Henry Davison are the unlikely capitalist heroes in this socialist drama. They failed, because they did not possess sufficient political influence in Washington. Had they succeeded, however, relations with the United States might have taken a very different turn, because the Soviet government was prepared to offer American business "far-reaching concessions" in developing the Russian economy. Clearly, the "spirit of Vladivostok" is the chief inspiration of this book.

The work is based primarily on *Foreign Relations of the United States* and, to a much lesser extent, the microfilmed records of the State Department. As a result, the first three chapters are unlikely to be of

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much interest to Western readers. In his last chapter, however, Ganelin also uses significant Soviet archival material to show the way in which the fledgling commissariats of the new regime viewed possible economic cooperation with the United States. This material allows him to take exception to George Kennan's argument that Soviet proposals for economic cooperation were nothing but "bait" designed to attract American capitalists into competition with their Japanese and German rivals in the Russian market. Elsewhere, however, he finds himself in frequent agreement with his American colleague and makes good use of Kennan's work to obtain interesting details which otherwise would have been unavailable to him. In places the organization of Ganelin's work even parallels that of Kennan.

Ganelin has presented a much more careful study than previous Soviet writers. His understanding of the period and his moderation in the use of language set him apart from them. Even he, however, can not avoid acute embarrassment when confronted with the ghost of Leon Trotsky. The Foreign Commissar is not even mentioned obliquely until halfway through the book, and the few references to him are all pejorative.

RICHARD K. DEBO
Simon Fraser University

WASSILIJ ALEXEEV and THEOFANIS G. STAVROU. *The Great Revival: The Russian Church under German Occupation*. Minneapolis: Burgess Publishing Company. 1976. Pp. xvi, 229. $21.95.

This work is an account of the struggle of the Russian Orthodox Church for survival within the Soviet system. Focusing on the years of 1941–43, and the religious revival which took place during that time on Soviet territories occupied by the Germans, the authors argue that this particular period is a "crucial interlude" (p. xi) in the modern history of Russian Orthodoxy. They stress, and rightfully so, its political, social, and cultural implications for the subsequent coexistence of church and state in the USSR.

The first chapter of the book deals with the events from the Bolshevik Revolution to World War II and sets the stage for the authors' analysis of the 1941–43 period. In both the introduction and the epilogue they try to assess the current status of religion in the USSR and conclude that today there is a "greater religious ferment in the Soviet Union than could have been anticipated a generation or so earlier" (p. 212).

The work offers concise accounts of the activities of leading Russian churchmen and provides numerous examples of the perpetual Soviet *Kulturkampf* and of the German *Kirchenpolitik* in Eastern Europe.

The book is well documented. In addition to archival material taken from such institutions as the Yiddish Scientific Institute and the U.S. National Archives, the authors include information obtained from interviews, letters, and private documents. All of these are highly revealing, but their reliability may not always be easily ascertained. Quite often even the names of sources are not provided (as in the case of the "intelligent, educated, and observant archpriest" [p. 77]), weakening at times the credibility of the study.

The most serious flaw of the work, however, is the authors' bias concerning the legitimacy and authority of Orthodox Churches other than Russian. When speaking of the Ukrainian Autocephalous Church, for example, the authors make references to the liquidation of this "separatist" Church, "along with other bourgeois nationalist movements" (note the use of Soviet terminology), and to the death of her "so-called bishops," who were, according to the authors, "schismatics" (p. 149). It is a shame that such "Great Russian chauvinism" mars what is otherwise a sound scholarly treatise on a very important subject.

LEONID RUDNYTZKY
La Salle College

VERA S. DUNHAM. *In Stalin's Time: Middleclass Values in Soviet Fiction*. New York: Cambridge University Press. 1976. Pp. xiv, 283. $16.95.

Vera S. Dunham's book is a detailed, scholarly investigation of the official and approved literary product of Stalin's time, and however unpromising that material may seem to us as literature, her book about it is a gem in its own right. Dunham, a sociologist as well as a literary critic, has created a book that gives sociological evidence an artistic shape and lightens the heavy matter with humor and human understanding. Her style is particularly enjoyable. I could easily fill several pages with her bright *bon mots*, her apt and original formulations, her sudden, illuminating generalizations.

The book deals essentially with that ugly phenomenon in Russian (and not only Russian) life known as *meshchanstvo*, which may be translated as "philistinism," though the Russian word carries a heavier charge of contempt and moral revulsion. The literature she examines is itself the product of *meshchanstvo*, and it promotes and propagates the values, if one can use the term, of Soviet *meshchanstvo*. An important side effect of her study is that she has shown that *meshchanstvo* is a deep force in Soviet life, with its roots in the Soviet system

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itself, but also in the past: "The resilience of *meshchanstvo*, its thrust and spread, suggest that it cannot be dismissed as peripheral, parochial, or spurious. On the contrary, it is of central importance to Soviet life. It is not monolithic because its origin is not solely Soviet. Having spread from two sources, the vestigial and the systemic, it currently possesses two aspects The old and the new *meshchanstvo* coexist" (p. 129).

The central thesis of the book is that at the end of World War II the Soviet regime entered into a compact (a "big deal"), "not with all of the people, but with some of the people," not with the proletariat or the intellectuals, but rather with a Soviet version of the "middle class," a social group that had come to power when it filled the vacuum left after the Great Purge of the revolutionaries. That "middle class" group was so attached to power, its perquisites, and its symbols that it feared nothing as much as change. In the difficult postwar years of reconstruction, the Stalinist regime depended on these "*meshchane*." It promoted and nurtured them, and in hungry and miserable Russia *they* enjoyed lavish creature comforts.

The book is a masterful exercise in the use of literary material as social and historical evidence. Dunham's most important and most original contribution is that she has provided us with a code for the interpretation of a literary product which induces boredom and disgust in most Western readers. She reminds us that these novels and stories were indeed *read*, and that the Soviet reader found something in them that he needed: models of profitable behavior, for instance, guidance as to the current morality or the relative importance of private and public loyalties, and, of course, indications as to the position of women. On this last subject let me quote Dunham's interpretation of the code signals she finds given in Soviet literature: "Every girl wants a happy married life . . . accented by the songs she will sing to her husband, and supported by her sixteen hours a day of work in factory, kitchen, and nursery."

The book illuminates incidentally such apparently extraneous matters as Soviet-American detente. The reference to Nixon and Kissinger does not escape us in the following reflection: "Those in the West who crave safety from adventures, who seek assurances against the unfathomable, and who fear unrest above all have begun to admire the bulwark of Soviet ruling *meshchanstvo*."

This is an original and unusual book, a first-rate contribution to our understanding of Soviet life and politics.

EDWARD J. BROWN
Stanford University

I. A. KHRENOV *et al.*, editors. *Dokumenty i materialy po istorii sovetsko-pol'skikh otnoshenii*. Volume 9, *Ianvar' 1940 g.–dekabr' 1949 g.* [Documents and Materials on the History of Soviet-Polish Relations, January 1940–December 1949]. Moscow: Izdatel'stvo "Nauka," 1976. Pp. 518. 3 r. 17 k.

Published in only 1,300 copies, this volume represents a joint effort between the Soviet and Polish Academies of Science. The time span covered is especially important, because it involved the elimination of all opponents and consolidation of a Stalinist-type regime in Warsaw, with Moscow's help. The USSR directed these activities, and in 1949 Marshal of the Soviet Union Konstantin K. Rokossovskii became defense minister of Poland and the symbol of this new relationship of subordination.

Such matters are not reflected in the book under review. It comprises 299 documents and other materials in the original Russian or translated from Polish on such matters as return of industrial machinery and libraries/archives confiscated by German occupation authorities, repatriation of Polish orphans and adults by the USSR (some even from Manchuria?), newspaper articles as well as speeches and letters by Warsaw government officials expressing gratitude to Soviet leaders, agreements on shipment to Poland of grain and textbooks as well as medical equipment, communiqués on visits by delegations, Polish-Soviet Friendship Society activities, statements by Stalin and Molotov about Poland's border with Germany, and a protocol on demarcation of the Polish-Soviet frontier, among others.

The volume includes separate indexes of names, places, organizations, periodicals, and newspapers as well as abbreviations used for archival collections cited in the text. A brief three-page introduction provides an overview of events, but the chronology on pages 453–65 appears to be more useful.

If a Western scholar were to rely on this publication alone, he or she would perceive the USSR as a benevolent power which selflessly gave what little it had at the end of the Second World War to help reconstruct a devastated Poland. Nothing appears in the documents about economic exploitation, e.g., shipments of coal from Silesia to the Soviet Union at $4.50 per ton (below cost of extraction) while Sweden was paying four times that much. Polish industrial plants that had been confiscated by the Germans were declared war booty and taken to the USSR or converted into joint stock companies, with the Soviets retaining a controlling interest of fifty-one percent.

The text of diplomatic notes on repatriation from the Soviet Union does not mention that 1.6 million Polish citizens had been deported to the USSR after the Hitler-Stalin pact which resulted in partition of Poland in September 1939. Many of

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the parents died in the Gulag Archipelago and, hence, the reference to orphaned children. The Hoover Institution at Stanford University holds release certificates, issued to some 47,000 forced labor camp inmates who were permitted to join General Wladyslaw Anders' army which later fought in the Middle East, North Africa, and Italy. Fortunately, primary source materials of this type do exist outside the USSR, and scholars in the West have access to information that presents the other side of Polish-Soviet relations.

RICHARD F. STAAR
Hoover Institution, Stanford University

MARTIN MCCAULEY. *Khrushchev and the Development of Soviet Agriculture: The Virgin Land Programme, 1953–1964*. New York: Holmes and Meier. 1976. Pp. xiii, 232. $24.00.

This is an important book for students of Soviet agriculture and politics. It is a thoughtful study of agricultural policy emphasizing the planting of the virgin lands of Siberia and Central Asia, which dominated agricultural policy and investment during the period from 1954 to Khrushchev's removal in 1964. The book provides an historical background to Russian settlement of these regions as well as a comprehensive picture of the problems of Russian agriculture—especially the desperate shortage of cereal grain production—that compelled new policy initiatives after Stalin.

The virgin lands program—overwhelmingly the central feature of Khrushchev's agricultural policy—receives the thorough treatment it deserves. Martin McCauley covers the program comprehensively, from its inception in a series of experiments in 1953 through its rapid development as a mature program comprising over forty million hectares of land. He concludes that "Khrushchev's bold démarche in going east in 1954 was correct" (p. 216), though the reader is given ample evidence of the negative side of the achievement in the form of crop failures and low productivity, soil erosion, and diversion of needed capital and labor from more fertile lands. The author also concludes that the other two major Khrushchev agricultural policies—the anti-*travopol'e* (rotation of grain and fodder crops) campaign and the widespread planting of corn—were "fundamentally correct." The error in all of the programs was precisely that they were conducted as campaigns: "Where Khrushchev went wrong was to take everything to excess" (p. 217). The task of his successors has been to moderate policy excesses, but the productive successes of the virgin lands during the post-Khrushchev years seem to vindicate his policy.

HERBERT J. ELLISON
University of Washington

NEAR EAST

MICHAEL W. DOLS. *The Black Death in the Middle East*. Princeton: Princeton University Press. 1977. Pp. xvii, 390. $19.50.

Plague was well known to Middle Eastern peoples long before the Black Death, as Europeans called the devastating pandemic which swept through the Eurasian continent in the middle of the fourteenth century. In successive chapters the author discusses, with a wealth of fascinating detail from Arabic sources: the history and transmission of the Black Death from its origins in the steppe lands of Turkestan; the nature of plague; medieval Muslim interpretations of plague; the demographic effects of plague in the Fertile Crescent; urban communal behavior during the Black Death; the economic consequences of this disaster; and recurrences of plague down to the Ottoman conquest of Egypt in 1517.

Plague itself may take three major clinical forms in animals and men: bubonic, pneumonic, and septicemic. Rats and fleas are the chief vectors; but neither in the Muslim East nor in the Christian West was the causal relationship between rats and plague recognized during medieval times. All three forms were prevalent in the Middle East during the Black Death and the recurrent plague epidemics. In both the Occident and the Orient the predominant etiological viewpoint, based on the humoral theory of disease elaborated by Galen, was that plague was produced and spread by corruption of the air, or miasma. The humoral theory was not only wrong, but subverted clinical observation. Moreover, orthodox Muslim reactions to the plague were conditioned by three major religious-legal tenets deriving from the *Qur'ān* and the *Sharī'ah* (Muslim Law): 1.) A Muslim should not enter into nor flee from a plague-stricken land. 2.) The plague is a martyrdom and a mercy from God for a Muslim, and a punishment for an infidel. 3.) There is no infection (contagion).

Both Christian and Muslim physicians and writers of treatises on the plague advocated what we now realize were usually futile and often harmful remedies for the dread disease. But Christian Europe did realize its contagious nature, and by taking rigorous quarantine measures did increasingly reduce the incidence of plague over the centuries, whereas Muslim passivity did not. The author argues that the apparently pacific, collective, and controlled Muslim reaction to the Black Death was largely the result of theoretical theological principles. For the Muslim, the Black Death was part of a God-ordered, natural universe; for the Christian, it was an irruption of the profane world of sin and an excruciating punishment.

Dols concludes that the Black Death sub-

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stantially reduced the Middle Eastern population, perhaps by more than one-third of its total by the early fifteenth century. More important, and in contrast to Christian Europe, there was no discernible recovery thereafter, owing to a number of adverse factors which the author closely analyzes, including a careful study of the case of the Mamlūk army. This well researched and cogently argued book is certainly a major contribution to the vast, if somewhat uneven, literature on the Black Death. It is unlikely to be superseded in its chosen field.

C. R. BOXER
University of Michigan, Ann Arbor

PAUL BALOG. *Umayyad, 'Abbāsid and Ṭūlūnid Glass Weights and Vessel Stamps.* (Numismatic Studies, number 13.) New York: The American Numismatic Society. 1976. Pp. 322, lv. $45.00.

This new book by Paul Balog constitutes an important addition to the heuristic tools essential for the study of Medieval Egyptian history. Basically it consists of a rigorously compiled and impeccably presented catalogue of over nine hundred specimens of glass weights and pharmaceutical measure stamps. This presentation is especially significant since it contributes another 40 percent to the corpus of known Umayyad glass and nearly 50 percent to the corpus of the 'Abbāsid and of anonymous weights and measure stamps. Also, the eight issues of the Ṭūlūnids in Balog's catalogue are all here published for the first time.

Apart from the catalogue and the fifty-five plates (good reproductions of specimens with legible inscriptions) the book contains an informative methodological and historical introduction. In it Balog sketches the story of earlier achievements in the area of Medieval Egyptian metrology, and offers a reconstructive analysis of the system of weights and measures, based on the interpretation both of survived specimens and on literary evidence. He also produces a detailed chronology of the officials whose names appear in the inscriptions on the glass weights and stamps. Finally, he provides his book with extremely useful indices in English as well as in Arabic: of the officials, of drugs, of pious legends, of honorary titles in the form of invocations. He thus makes the evidence extracted from the difficult and scattered body of such prosaic source materials easily accessible to interested historians.

This rich body of information by Balog marks another important step in modern methodological efforts to arrive at some statistical standards which would enable us to evaluate quantitatively the diversified operations of the Nile valley economy—the main determinant of the history of Egypt in the Middle Ages.

ANDREW S. EHRENKREUTZ
University of Michigan, Ann Arbor

JOHN E. WOODS. *The Aqquyunlu—Clan, Confederation, Empire: A Study in 15th/9th Century Turko-Iranian Politics.* Chicago: Bibliotheca Islamica. 1976. Pp. x, 348. $20.00.

John E. Woods demonstrates in his work on the Akkoyunlu the excellent results obtainable when a researcher is well trained both as an Orientalist and as a historian. His notes and bibliography show deep familiarity with a wide range of sources, both original and secondary, in Arabic, Persian, and Turkish. European sources, especially Venetian, are in evidence also, but this work could not have been written without access to the indigenous Near Eastern materials. Woods is to be congratulated for his assiduous use of these manuscripts, documents, and printed works, each of which contains language pitfalls that can cause one to lose heart and give up in despair. His perseverance alone merits our acclaim.

There is, however, more to admire here than personal fortitude. Woods has managed to produce a coherent and consistent political narrative from sources that are often biased, incomplete, inaccurate, and contradictory. Much was at stake in a region comprising eastern Anatolia, western Iran, and parts of Syria and Iraq. There the Ottomans, Mamluks, Timurids, and Turkman tribes contested for dominion. Boundaries were fluid, alliances mercurial, treachery rampant, and leadership alternated between being charismatic and spasmodic. Out of this cauldron emerged the Akkoyunlu to exercise hegemony over an ever-widening area. Woods follows this fascinating story from its inception in the mid-fourteenth century to its conclusion at the beginning of the sixteenth with the rise of the Safavids. The chief protaganist is Uzun Hasan who cast himself in the role of an Islamic world conqueror in the mold of Tamerlane.

Uzun Hasan adhered to the political philosophy that treated sovereignty as a family affair. During their lifetime such rulers parcelled out apanages to royal princes, with succession usually being decided by the will of God through civil war. Woods describes this system in detail, but offers no explanation as to why it was adopted and adhered to in the first place, and why succession was not regulated in a more orderly fashion in order to provide for a more stable political order. Perhaps that is too much to expect in one book.

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In his final section Woods contributes to our knowledge of how Shah Ismail was able to convert a moribund Sufi order into a militant, expanding Shi'ite state by detailing the political disintegration in the area after the collapse of the Akkoyunlu. Much of what Woods has to say here and elsewhere in the book was influenced by his teacher, Martin Dickson. We are indebted to both of them.

NORMAN ITZKOWITZ
Princeton University

KONRAD MORSEY. *T. E. Lawrence und der arabische Aufstand 1916/18.* (Studien zur Militärgeschichte, Militärwissenschaft und Konfliktsforschung, number 7.) Osnabrück: Biblio Verlag, 1976. Pp. xi, 466.

The fiction, facts, and legends surrounding the role of T. E. Lawrence in the origins of the Arab Rebellion and of World War I are so expansive, controversial, and numerous as to become epidemic. One would feel that nothing of consequence could be contributed to the lore of Lawrence. Konrad Morsey's book, based on his dissertation completed at the University of Münster, refutes this view in several significant respects. Morsey concentrates, more than others (Kedourie, Dawn, Monroe, Friedman, Mousa, Sacher, Zeine, Lenczowski, Busch, Antonius, Kirk) on the so-called "Rabegh Crisis." Rabegh is a seaport on the main thoroughfare between Medina and Mecca. The Ottomans had to pass it in a counter-attack to relieve British pressure on besieged Medina and to gain a prestigious victory. Such a triumph would give the British pause concerning a possible Ottoman (German) threat to Egypt, Sudan, and the two littorals of the Red Sea. "Rabegh" became a code word to describe the crisis lasting from mid-October to mid-January 1916. The "Rabegh Crisis" demonstrates the confusion caused by the ambiguity of Hashemite and British objectives and the subsequent accusations of each party and their partisans as to the duplicity in which each engaged. Did Faysal need troops to prevent a possible Ottoman counteroffensive or did he attempt to use the threat of an Ottoman attack to pry more soldiers from the British (and French) for an attack inward from the coast to create greater support for his own ambitions? T. E. Lawrence was sent to determine whether or not additional troops were needed at Rabegh.

In Morsey's view Lawrence went to Rabegh (an important matter in the creation of the Lawrence legend), performed his mission successfully, and as a result was thrust to the fore of British agents working with the Arabs. Lawrence was catapulted further into the limelight by the unexpected conquest of Aqaba as a result of the Auda-Howaytat war in July 1917 which made it necessary for the British to create the "Northern Army," led by Faysal, to protect the flank of Allenby's Palestinian Army. Lawrence went north with Faysal into Syria.

In Morsey's opinion Lawrence was not the "uncrowned king of the Arabs" but, indeed, his influence in the Arab Rebellion was great. The author attributes Lawrence's success to the fact that he was one of the first theorists of limited and guerrilla warfare. As a theorist of limited war he was, unlike that other theorist of limited war, Henry Kissinger, able to implement his ideas successfully on the battlefield. Morsey's book should be of special interest to American military historians, since Lawrence's ideas of guerrilla warfare, at least up to 1968, were still given credence at the School of Guerrilla Warfare in Fort Bragg, N.C. Apparently the Americans used some of Lawrence's ideas in Vietnam, not with the same success, however, that Lawrence had in Arabia.

The Arab Rebellion has become bogged down in polemics, even in the works of serious historians, about who was the most deceitful, Arabs or British. This unfortunate, although understandable, situation has been exacerbated by the entry of Israeli and Jewish (especially British and American) historians into the fray, usually equally against Briton and Arab. Perhaps an "outsider" was needed to restore some perspective and balance to the origins of the rebellion and the role, character, and significance of T. E. Lawrence as a soldier, hero, theorist, and practitioner of guerrilla warfare. T. E. Lawrence fares very well.

This important revisionist work, enhanced by a judicious critique of some of the best works on Lawrence and the Arab Rebellion, deserves a large audience. Because of its interest to historians of the Middle East, and to military and diplomatic historians, it should be translated into English.

ROBERT OLSON
University of Kentucky

HALIM BARAKAT. *Lebanon in Strife: Student Preludes to the Civil War.* (Modern Middle East Series, number 2.) Austin: University of Texas Press, 1977. Pp. xiii, 242. $12.95.

ENVER M. KOURY. *The Crisis in the Lebanese System: Confessionalism and Chaos.* Washington: American Enterprise Institute for Public Policy Research, 1976. Pp. 92. $3.00.

The most compatible element found in these two studies concerns the political situation in Lebanon. Both authors state that the existing "Confessional" system, where church and state are inseparable, is anachronistic and that serious

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modifications, or an entirely new form of government, is required. With the nonassimiliation of the religious communities, Lebanon continues to be a mosaic society. In the past, such an unstable situation often resulted in violence, thus the seemingly inevitable hostilities of 1975–76. However, given the various political orientations of these sectarian communities, the question of religion as the basic determinant of these hostilities still remains rather enigmatic. Is it possible to describe recent hostilities merely in terms of Christians versus Moslems?

Certainly religion and its influence on Lebanese society and politics should not be completely ignored. In his survey of university students, Halim Barakat concluded that these particular Lebanese were not liberated from any of their traditional religious loyalties; thus religion was the primary influence in the determination of their political preferences. This influence, accompanied with strong familial legacies, even prevailed over economic and social-class pressures. Religion serves as a fundamental factor for these university students in such issues as political reform, social alienation, and attitudes toward the Palestinian movement. It is therefore not surprising to findthat affluent Moslem students, even more so than poor Christian students, preferred leftist-oriented politics. Might we conclude that the recent hostilities occurred between leftist Moslems and rightist Christians?

Although this point seems valid, Enver M. Koury argues that it is incorrect to assign exclusively leftist politics to Moslems and rightist politics to Christians. After all, twenty different factions participated in the recent hostilities. In fact, Christians from the leftist Syrian Party fought their coreligionists from the *Kita'ib* (*Phalange*), while the Palestinian Moslems of *al-Saiqa* joined the Syrian army in repressing their brothers in *al-Fatah*. Therefore, the author prefers to view the 1975–76 hostilities as a struggle between rightists and leftists, rather than between Christians and Moslems.

In addition, Koury has correctly identified the *za'im* tradition as the major obstacle to political reform. If these archaic feudal lords, such as the former president, Suleiman Franjieh, were liquidated as a class, then political reform could succeed. Unfortunately, on another major problem, the author hesitates to offer a solution: he eschews the Palestinian question, simply hoping that they will peacefully depart from Lebanon. Of course, given the intransigent attitudes of the Zionists, this solution remains impractical. This latter point is a major shortcoming in Koury's study.

Similarly, there are some limitations in Barakat's work. Basically, the absence of a concluding chapter which would summarize his research findings is slightly annoying. More important, he has unsuccessfully attempted to relate his findings to the recent hostilities. Although this idea is a worthy one, it proves impractical, as his survey concerns the attitudes of university students only during peacetime. In reality there are few similarities between these university students and the active participants in recent hostilities. If this author desires to conduct research on student-fighters in 1975–76, he must ignore the universities and concentrate on the secondary schools. One hopes that future studies will clarify this particular point, and thus extend the usefulness of Barakat's basic findings.

W. DAVID WRIGLEY
Seabrook, Maryland

AFRICA

ROLAND OLIVER, editor. *The Cambridge History of Africa*. Volume 3, *From c. 1050 to c. 1600*. New York: Cambridge University Press. 1977. Pp. xiii, 803. $48.50.

The appearance of this volume continues the seris of the *Cambridge History of Africa*, and joins volumes 4 and 5, which have already appeared. Roland Oliver, the editor of this volume is aided by a considerable array of star talent. He himself has written the last chapter on the "East African Interior." The other eight were done by Ivan Hrbek, "Egypt, Nubia, and the Eastern Deserts," Taddesse Tamrat, "Ethiopia, the Red Sea and the Horn," H. N. Chittick, "The East Coast, Madagascar and the Indian Ocean," H. J. Fisher, "The Eastern Maghrib and Central Sudan," N. Levtzion, "The Western Maghrib and Sudan," J. D. Fage, "Upper and Lower Guinea," David Birmingham, "Central Africa from Cameroun to the Zambesi," and D. Birmingham and Shula Marks, "Southern Africa."

To take these chapters in turn, Hrbek has written a conventional yet fairly complete and comprehensive study of the Fatimid, Ayyubid, and Mamluk dynasties of Egypt. He adds material on Nubia, the Beja, details of the Arabization of the Sudan, relations between Egypt and Ethiopia—particularly the dispatch of Coptic clerics to Ethiopia—and with the Karimi merchants on the Red Sea and Indian Ocean. He also covers Egyptian contacts with East Africa and the Horn, discussing the slave trade, Zayla', and the "Islamic Fringe" states in Ethiopia which had trade relations with Egypt. He comments on the general lack of Egyptian Muslim influence in East Africa, citing as the

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only example the use of regalia, parasols, and heraldic animals (here the same material appears on both pp. 85 and 94). Hrbek has some useful comments on the economic decline of Mamluk Egypt (pp. 63–64), and some interesting details about the import of Turkish and Ethiopian slave girls and eunuchs into Mali.

Taddesse Tamrat's chapter is also a very complete one. Tamrat gives a full statement, with some new and interesting detail, of the fall of the Zagwe and rise of the Solomonic dynasties, with their interworkings with the Muslim states of the coastal plain and the east side of the plateau. He emphasizes the role of the Dahlak Islands and Zayla' as intermediaries between the Muslim world and the highlands, likewise the significance of Zabid in Yaman, not only as the site of a "Hagshi" dynasty, but also as the main medieval trade and slave port for the east side of the Red Sea. Tamrat lays to rest the ghost of Queen Judith of the "Banu Hamwiya," who is shown to have been the Queen of Damot. It would have been interesting to know more about the Shoan state of the Banu Makhzum, presumably *sharifs* of the Quraysh, and of its trade and other connections to Mecca. Tamrat also includes here a full account of the expansion of Christian Ethiopia from 1000 to 1150, and again from 1376 to 1468, at which time he switches to the Muslim revival and the campaigns of Ahmad Gran (ending with the latter's death in 1543). The chapter ends with an account of European (primarily Portuguese) involvement with Ethiopia, mostly in the early sixteenth century.

Neville Chittick's chapter on the East Coast, Madagascar, and the Indian Ocean deals with the first Muslim contacts there, the establishment of towns and trading posts, the gradual development of a "Swahili civilization," part of a huge maritime trading network linking East Africa to the Persian Gulf and to Indonesia, as well as to the Indian subcontinent. Chittick, an archeologist, furnishes a lot of new information based on his own excavations and those of others. Among other things, he discusses the medieval ivory, textile, gold, and slave trades, concluding that the textiles arrived largely from India, were probably of cotton, with some silk, along with pottery and beads from various places, including a large proportion from the Far East. Chittick really does not commit himself about the medieval slave trade from Tanzania, Kenya, and farther south. There is evidence to suggest, however, that it was occasionally lively: a ship from Qays in the Persian Gulf was equipped for a slave raid in the thirteenth century and brought back large numbers of captives. Finally, Chittick makes the point that the Muslim "city-states" of coastal East Africa were "isolated from the mainstream of events, preoccupied with their own affairs, chiefly . . . the making of money" (p. 219). This is a distinguished chapter.

Fisher and Levtzion discuss the eastern and western Maghrib and corresponding regions of the Sudan. In his section, Fisher includes a discussion of "Iron, Horses, and Guns" a useful account of the changes brought about by these three factors, and tries to trace them. The penetration of Islam in the area whether by clerics and *sharifs*, (probably slightly overdone here), education, or the pilgrimage, is another useful theme treated by Fisher. The chapter by Levtzion gives a useful account of the two Berber empires (Almoravids and Almohads), a discussion of the origins and development of ancient Ghana, and evidence that the growth of trade and a merchant class, state formation, and the spread of Islam are closely interrelated. Levtzion is equally succinct and competent with his account of the political domination of Mali (c. 1260–1360) in the western Sudan. Levtzion also includes a comprehensive account of the "maraboutic crisis" of late-fifteenth-century Morocco, to which he brings some new insights. The chapter closes with an account of Songhay and some details of the states of Mamprusi, Dagomba, and the Mossi.

"Upper and Lower Guinea" are the provinces of J. D. Fage. "Orientations and a Prolegomena" at the start of the chapter contain much useful and suggestive information, such as details of metal working and casting at Benin and Yorubaland, the use of horses in this tsetse-ridden region, the influence of a savannah vs. forest environments, and the significance of the "Dahomey Gap" for social, political, and agricultural developments. Fage also comments on the impact of the first Europeans in West Africa, when and where they arrived and how they effected a transformation of Guinean societies. Fage does a very useful service in his criticism of E. L. R. Meyerowitz and her claims (see p. 491, and note 1). The Portuguese, the Dutch, the English, and the themes of firearms and Christianity are well dealt with here, too.

David Birmingham writes about Central Africa from the Cameroun to the Zambesi in chapter 7. In chapter 8 he has the competent assistance of Shula Marks in tackling the complex themes of "Southern Africa." Central Africa, that underpopulated and under-researched region, is shown to have had distinct iron-working and pottery traditions, and interesting economic and political relations between forest and savannah cultures, but very little contact with the outside world, and few innovations of any consequence, even fewer markets or industries. However, the Sanga people of southern Zaire developed interesting copper-working techniques and exported copper ingots, presumably to the East African coast. Ingombe Ilede

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on the upper Zambesi is another fascinating site, a transfer point for other copper wares mined nearby, and often sold with ivory, for coastal products like cloth and beads. Another theme, less well known but significant, is the *ngola* ritual symbols of iron among the Ndongo, Kimbundu, and Samba of Angola, with the growth of the *ngola*-chief institution. Among other matters there is a discussion on Gaoga and its possible site, its economic linkages, and its "slave catchment area in the Ubangi basin." Birmingham gives a fine narrative here of ongoing disruptive change in this region, and its gradual penetration by foreign influences.

The section on the "Northeast" in the Southern Africa chapter contains some exciting new material on Great Zimbabwe. Its wealth was derived from external long-distance trade: as Marks and Birmingham say, "the wealth of Zimbabwe flourished for two centuries only (c. 1200–1400). . . . The time-span is exactly the same as the time-span of Kilwa's prosperity, and the link between the two seems almost certain" (p. 580). Further, Birmingham declares that control of trade via the Sabi Valley to the sea "could have financed the construction of Zimbabwe's monumental architecture" (p. 581). Indeed, he and Peter Garlake have probably found that outlet (Mantkwtni), a sub-Zimbabwe on the Mozambique coast, just south of the Sabi mouth, but not recorded in this chapter. Then, too, can the royal fires of the Barwe—on the occasion of New Year—be of Iranian inspiration? The Sassanians had similar customs.

In the last chapter Roland Oliver discusses the East African interior, the Iron Age and iron-working there, migrations, cattle-keeping, and linguistic matters, salt collection, pottery, and as a final subject, the "coastal hinterland." This historically mysterious region, as narrow as 80, and as wide as 320 kilometers, lying just west of Tanga, Mombasa, Dar es-Salaam, the Rufiji Delta, and the Lamu Archipelago, must have supplied the slaves, the ivory, even the food of the coastal Islamic communities for a very long time. And through their territory passed long-distance trade routes, not only in the eighteenth century—but probably long before that. But as Oliver says, "of their earlier history we know, as yet, nothing."

This is a very fine volume. The chapters are uniformly good. It is well printed and there are enough maps to follow the textual discussions. But this useful production is marred by the Cambridge University Press' excessively high price, meaning that only libraries can afford copies—or that an occasional reviewer gets one by chance.

B. G. MARTIN
Clare Hall,
Cambridge University

HAROLD G. MARCUS. *The Life and Times of Menelik II: Ethiopia, 1844–1913.* (Oxford Studies in African Affairs.) Oxford: Oxford University Press. 1975. Pp. xiii, 298. $17.00.

This is not an easy review to write, for it is all too tempting to criticize the author for what the book is *not*, rather than for what it is. Harold G. Marcus, who has written extensively about various aspects of modern Ethiopian history, has spent much of the past dozen years undertaking the research for this study. By his own admission, he had originally intended to write a compleat biography of Menelik the man, as well as Menelik the emperor. Yet despite his thorough inventory of the holdings of the archives of the colonial and foreign ministries of Britain, France, and Italy, his survey of Ethiopian manuscript and published materials, his knowledge of the extensive bibliography of Ethiopian studies, and his attempt to interview octogenarian survivors of Menelik's court, Marcus ultimately discovered that he could not hope to meet with success in writing a biography of Menelik in the style of the great biographies of Menelik's European contemporaries. Accordingly, he wisely tailored his study to enable him to work within the limitations of his materials.

Menelik II, king of Shoa (1866–1889) and then emperor of Ethiopia (1889–1913), must certainly rank among Africa's greatest leaders. As a bridge between the precolonial and colonial eras, he had the satisfaction of inflicting a major military defeat on a European power in 1896 at the famed battle of Adowa. Skilled in the traditional politics of a feudal society, Menelik grew in foresight as he grew in power. As king of Shoa he was fascinated by the gifts brought to him by French, Italian, and British emissaries; but as emperor of Ethiopia he had the vision to see how nineteenth-century technological marvels like the railroad, the repeating rifle, the telegraph, and the telephone might strengthen the infrastructure of his state. A warrior who could mobilize an army of more than one hundred thousand on a few day's notice, Menelik was able to extend his sway from over his patrimonial lands in Shoa to incorporate into the Ethiopian empire huge areas to the south and east in an impressive sweep of imperialist expansion that discomfited the European powers which had established themselves on the borders of his greater Ethiopia.

The rise and decline of Menelik have elements, too, of classical tragedy. Marriage alliances were as important for him as for the Habsburgs, yet he never produced a male heir. At the height of his power the awesome monarch was felled by a series of physical ailments culminating in strokes that left him—and his newly modernizing state—paralyzed. The impressive accomplishments of his ear-

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lier years were not sufficient to ward off the centrifugal tendencies of his own court. The Empress Taitu and then regional warlords manipulated the strings of government until the old emperor's designated successor, his young and politically inexperienced (some would say debauched) grandson Lij Iyasu, attempted to rule as an uncrowned monarch. The system that Menelik had painstakingly assembled, part feudal and part modernizing, crumbled, and Ethiopia slipped into a quasi-civil war until a coup placed Menelik's daughter on the throne and designated young Ras Tafari (later known as Haile Selassie) to serve as regent.

Unlike earlier works of Ethiopian history which viewed Menelik from the perspective of the European empire-builders, this biography succeeds in placing the emperor squarely within the Ethiopian context. For Marcus Menelik's life and achievements represent a historical process that shaped prerevolutionary twentieth-century Ethiopia. The themes are clearly stated in the introduction: Shoan expansionism to obtain control of the resources and trade of the south, Menelik's abiding appreciation of the need for territorial and political unity, his recognition of the military and political value of modern technology, and his statemanship and remarkable skill in diplomacy as he adroitly balanced regional forces within Ethiopia and deftly parried the thrusts of the European imperialist powers on its borders. Again and again, Menelik's creativity in governmental administration and his consummate political sense in international relations are depicted with a view to demonstrating the old emperor's sense of destiny.

The evidence is lacking as to what Menelik actually thought of his role in Ethiopian or African history. This Marcus was unable to unearth. Nevertheless, with its richness of political detail compensating for the paucity of nonpolitical biographical material, *The Life and Times of Menelik II* provides a crowded panoply of events and personalities that leave the reader with the feeling that he has absorbed a very great amount of information. Yet the reader new to the almost Byzantine quality of Ethiopian history may at times feel lost. There is no glossary of the Ethiopian terms which are sprinkled throughout the text, nor is sufficient background material provided for the nonspecialist. Occasional errors of fact and inconsistencies in transliteration also mar the text. But on the whole, the book, while not definitive, will be widely useful for some years to come.

ROBERT L. HESS
University of Illinois, Chicago Circle

RAPHAEL DANZIGER. *Abd al-Qadir and the Algerians: Resistance to the French and Internal Consolidation.* New York: Holmes and Meier Publishers. 1977. Pp. xv, 300. $29.50.

Few attempts by non-Western nations to create national heroes from within the ranks of leaders who resisted European penetration have been more intensive or successful than the campaign to portray Abd al-Qadir as the progenitor of Algerian nationalism and, thus, of the state which came into being in 1962. Marxist historians have facilitated this endeavor, although many other recent scholars have depicted Abd al-Qadir as a religious leader devoid of a sense of "Algerian-ness." Balancing the frequently idealized and romanticized biographies and hagiographies of the amir with considerable archival material and the study, in Arabic, of Abd al-Qadir by his son, Raphael Danziger scrutinizes the various interpretations and offers one of his own in this admirable study of Abd al-Qadir from 1832 to 1839, his most critical years.

The author's major conclusion is that Abd al-Qadir was a shrewd, highly pragmatic, political leader who utilized the Islamic duty of *jihad*, or holy war against the infidel, as one mechanism, but neither the exclusive nor overriding one, for rallying resistance against France. The threat to Islam had not, in itself, been sufficient to provoke a spontaneous anti-French movement after the fall of Algiers in 1830, any more than a notion of Islamic solidarity had been able to obliterate substantial antagonisms between Turks and Algerians prior to that time. Abd al-Qadir's initial appeal to the tribesmen who gave him their support in 1832 lay in his potential for ending the intertribal strife and general anarchy which had permeated western Algeria's hinterland in the preceding two years. The amir's subsequent negotiations and treaties with the French; his tendency to appoint as his agents and deputies men with political experience and local power bases, including many who had collaborated with the Turks, rather than members of the religious nobility; his continuation of non-Quranic taxes to support his undertakings; and the wholesale desertions he experienced in times of crisis all suggest that neither he nor his followers put the religious nature of their activities uppermost in their minds.

Throughout the book, Danziger refers to Abd al-Qadir's "state" which, he notes, is a term of objective reality which lacks the subjective connotations of "nationalism," "nation," or "patriotism." The book provides numerous examples of Abd al-Qadir's refusal to yield sovereignty over Algeria's interior while insisting on the retention of his own prerogatives. A series of appendixes giving Arabic, French, and English versions of Abd al-Qadir's treaties with the French reinforce these examples,

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making a convincing case for Abd al-Qadir as a state builder. His success, however, was in consolidating the tribes without solidifying them, thereby taking the first step toward the formation of a nation but not, as some would have it, engendering a sense of national sentiment.

A few annoying slips do flaw this work. The Moroccan sultan is described as anxious over the thought of a European power entrenched across his *western* border (p. 43), and a French expeditionary force going from Constantine to Algiers suddenly appears to be marching in the opposite direction (pp. 165–66). Nevertheless, this is an important work greatly elucidating a case study in indigenous resistance.

KENNETH J. PERKINS
University of South Carolina

EMMANUEL SIVAN. *Communisme et nationalisme en Algérie, 1920–1962.* (Travaux et recherches de science politique, number 41.) Paris: Presses de la Fondation Nationale des Sciences Politiques. 1976. Pp. 261.

According to the author, this study is not a history of Algerian Communism. Nor is it a formal history of this political party in Algeria between 1920 and 1962. It is at once much more and less than that; it is an excellent description of the complex relationship between Communist theories and practices, on the one hand, and colonial realities, the problems of decolonization and of native nationalism, on the other. It is also a study of a minority party within the European community of Algeria, which itself represented less than ten percent of the total population, but was politically, socially, and economically dominant. Working to undermine the French order and to substitute itself at the head of the government, the Algerian Communist Party found itself sandwiched between the authority it would replace and the Algerian nationalism which sought to free the country from foreign control. Finally, this book is a history of the attempts of this small group of political activists to become Algerian.

Algerian Communists were never more than a minority party. They never achieved power, were never really trusted by Algerian nationalists, did not manage to become an indigenous party, and, to the end, suffered mostly because they could not shed the image of themselves as a European—i.e., foreign—party. Most of the members of the party, whether they recognized the fact or not, were members of the dominant group in a dominant/dominated situation such as that described earlier by Albert Memmi in *The Colonizer and the Colonized* (1965). Had they managed to become Algerian, Communists in Algeria would still have been suspect because their ideology was imported.

Emmanuel Sivan's book is an excellent and essential study of various aspects of colonialism and anti-colonial nationalism, and of how a particular Communist party interacted with these political movements. The author proves, beyond a doubt, Memmi's thesis that there was no possible compromise between the colonizers and the colonized. Even Communists who were proletarians and, as such, should have been closer to "average" Algerians, were colonizers, dominators. They could not become Algerians. What Memmi had felt and clearly described might be dismissed as propaganda, but, as demonstrated by Sivan, his thesis cannot be refuted; the available evidence which he has gathered and interpreted is overwhelming.

Sivan explores several problems that should, when solved, continue to offer interesting results: the problem of minority parties of dedicated members who try to replace established orders—whether in a colonial situation or not. He also touches on the existence of the "slaveholder mentality" which developed among European Communists in Algeria, among people who should have been, because of their party's ideology, anti-colonial zealots. These are two of several concepts that might profitably be applied to situations other than that of Algeria. All told, Sivan's study is a welcome addition to historical literature, and should serve as a model for other specific studies that will enable us finally to get a good synthetic interpretation of the history of dominant Europeans overseas and of native reactions to foreign rule.

Some small complaints might be addressed to the author. There is, for example, no index and no systematic bibliography, although the sources are outlined and described in the introduction and in the footnotes. Nevertheless, the value of his study on the interpretive level is undeniable. *Communisme et Nationalisme en Algérie* is, and will remain, essential reading for all who wish to understand Communism in the Third World, the problems of colonialism and of decolonization, and, more specifically, the relationship of Communism to anti-colonial nationalism in countries in which there were strong settler populations.

ALF ANDREW HEGGOY
University of Georgia

A. I. AŞIWAJU. *Western Yorubaland under European Rule, 1889–1945: A Comparative Analysis of French and British Colonialism.* (Ibadan History Series.) Atlantic Highlands, N.J.: Humanities Press. 1977. Pp. xvi, 303. $13.00.

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This volume stands as a case study of one area in which the debate over similarities and differences in French and British colonial rule can be historically tested. A. I. Aşiwaju has carefully analyzed the intrusion and effects of the two major colonial powers into Western Yorubaland, which eventually was made part of French controlled Dahomey and British controlled Nigeria. There is no doubt concerning the author's conclusions: in every domain he demonstrates that the British did better, both with respect to their own dominion and the continuation of Yoruba institutions, than did the French.

In one of the most effective sections of the study, the author traces the initial involvement of both colonial powers and describes their similarity in purpose and technique, and—above all—in the reception they received. The Yoruba welcomed the Europeans as providers of a local *pax*, assuring conditions which they themselves could not achieve. And therefore it was only with the articulation of colonial policy that reactions changed in general, and in particular according to which colonial power the Yoruba found as their temporary master.

After this well-constructed and somewhat revisionist assessment, the author leads his reader through the major institutions. Here the quality of the chapters varies as indeed might be expected. The chapter on the effects of colonial rule on the chieftaincy institution is a fine analysis, adding more knowledge to the major historical issue of the meaning of "indirect rule," in both the French and British contexts. The chapter on "civil obligations" stresses a new aspect of resistance, emigration, with a good analysis of the migratory developments that took place from Dahomey to Nigeria in the interwar period. But the chapters on social institutions, education, and religion are more routine in scope and presentation.

Although some reorganization to avoid the brief chapters in the latter portion of the book might have allowed for a stronger narrative, this study stands as a significant and well-considered contribution to the literature on colonialism in Africa. Most important, it offers new reason to support the old argument that the effects of French and British colonial rule were more different than similar. Certainly they were in Western Yorubaland, as Aşiwaju amply proves.

RAYMOND F. BETTS
University of Kentucky

D. A. LOW and ALISON SMITH, editors. *History of East Africa.* Volume 3. New York: Oxford University Press. 1976. Pp. xii, 691. $36.25.

This is the long-awaited third, and apparently final, volume of the Oxford history of East Africa first planned in 1956. The changes in editorship, problems with source material, substitutions among authors, and evolving concepts of history that occasioned the long delay in producing the third volume are explained in the preface. The work generally adheres to the pattern of the earlier volumes. The main political and economic developments of Kenya, Uganda, Tanganyika, and Zanzibar for the years 1945–64 are covered in the first eight chapters, each under separate authorship. A description of the East Africa Royal Commission of 1953–55 and statistics on population, trade, economic development, and representation in the territorial legislatures form the content of three appendixes. There are also six maps, a select bibliography, and an index.

In one essential the third volume is unique. The last six of the fourteen chapters are devoted to themes that were neglected in the first two volumes. The East African legal system, Christianity, Asians, Europeans, Arabs, and changes in African society are the successive topics. Unlike the preceding, these six chapters cover longer periods, most going back to the beginning of the British presence about 1890, but one, the chapter on the Arabs, boldly embraces "the past millenium."

Though this material is ostensibly designed for the "general reader," there is likely to be no writing on East Africa more valuable to the specialist. Judging from the use of volumes 1 and 2, these are the first books purchased for the scholar's private bookshelf, and despite attempts by the editors to enliven the style, they are avoided by the general reader in preference for the less factual, more exciting—often sensational—personal and journalistic accounts for which East Africa offers so much opportunity.

Examined from a scholar's perspective, this third volume has some major shortcomings. References to Asians, who were so important to economic development and to cultural conflict, are conspicuously lacking in the first eight chapters, and the one chapter (12) devoted exclusively to them, though a valuable addition, does not correct this distortion. The African contribution to the postwar evolution of East Africa, primarily economic but also political, receives inadequate attention in the eight chapters, and the one on African society (14), though imaginative and informative, does not offer full compensation. Mwindi Mbingu, the Kamba protest leader for whom a prominent street in Nairobi is named, is not mentioned anywhere, and other leading Africans could be added to the list of omissions. African women and trade unions are other neglected topics, and "underdevelopment," now the subject of major con-

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troversy, is not critically examined. It is unfortunate, too, that no African was included among the authors and editors.

There are also a few minor flaws. The East Africa Royal Commission of 1953–55, which admittedly produced no important change, does not seem deserving of an appendix. The personal style of most authors, including not only the artificial "we" and "us" but the chatty "our period" (e.g., pp. 290, 320), detracts from the overall scholarly quality. The statistics of two closely related, important tables (pp. 291, 588) are not coordinated, and the reference years for chapter 6 are omitted in the title (p. 251).

It is comparatively easy and perhaps unfair, however, to levy criticism from a vantage-point nearly two decades after the work was first planned. Margery Perham, Alison Smith, D. A. Low, and the others who figured prominently in the planning and production could not have foreseen many of these things. On the whole they have made an extremely valuable contribution with a wider significance than the area of focus. This volume, like the preceding two, is pertinent to the history of the British Empire and European expansion as well as East Africa. Most of the authors are among the foremost authorities on the subjects of which they write. Many of the chapters, such as 7 on the Tanganyika economy by Cyril Ehrlich, and 9 on the legal system by Anthony Allott, are not just compendiums but articles with a unique and enduring substance.

ROBERT G. GREGORY
Syracuse University

ASIA AND THE EAST

HOLDEN FURBER. *Rival Empires of Trade in the Orient, 1600–1800.* (Europe and the World in the Age of Expansion, number 2.) Minneapolis: University of Minnesota Press. 1976. Pp. xix, 408. $18.00.

In his new work of synthesis and interpretation, Holden Furber gives us the fruits of almost half a century of work in Indian-European relations. He has transcended his personal specialization on eighteenth-century India to survey as well the seventeenth century and European encounters with Japan, China, and the Malay-Indonesian world; in all this he shows the most impressive mastery of materials on the English, French, Dutch, Danish, and (to a lesser extent) Portuguese activities in Asia, 1600–1800. Of course, as he himself admits in the introduction, his own research interests are reflected somewhat in the weighting he gives to topics and in the relative mastery of subject matter in different chapters. I thought chapter one, on the decades 1600–60, the least satisfying in the book, far too much a catalogue of personal and place names, too many only mentioned once. The later chapters grow markedly better. In general, I was more impressed with the analytical part two than with the narrative and multi-focused part one. In analyzing (in part two) the various East India Companies, their trade, their servants, and their place in Asian society, Furber shows admirable mastery of most intricate subjects and a congenial mode of approach that integrates the experience of the different companies. To this reader the most masterful seemed to be chapter six, on the "country trade" and the rise of the "agency houses."

The publishers have produced a handsome book with interesting illustrations and valuable maps—though I should have preferred more detailed maps, particularly of India. I also think that the volume could have benefited from some summary statistical tables and graphs—though perhaps the planned style of the series did not allow for this.

The emphasis throughout is on events in Asia, sometimes I think to the neglect of conditions in Europe that gave the various companies their monopolies. At the home end occurs one of the rare slips in the book: the yield of the domestic tobacco monopoly to the French Indies Company was significantly higher than the 4 million suggested on page 137; in fact, 6.8 to 8 million livres p.a. during 1723–47 and 9 million compensation for the loss of the monopoly thereafter.

Furber's great strength in institutional analysis does not quite carry over to economic analysis. The judgments he gives in the text and conclusion are always judicious, but we too rarely get an explanation of how he came by those conclusions. For example, on page 177, he appears to define the amount of wealth drained from India as the "*sale amount* [in England] of India goods (tea excluded) . . . *not requited* by exports from London to India." (He cites only N. K. Sinha, who in turn cites an earlier work by Furber as one of his principal sources.) Since the high figures given in this passage will inevitably be pounced upon by innumerable students and teachers using this book, they deserve fuller explanation. That there was some drain of wealth from India is not under question; what is doubtful is that it can be measured by subtracting the cost of outward cargoes from the receipts from the sale of return cargoes. From an accounting standpoint, just to break even, the amount of home sales plus the value of any bills of exchange remitted from the East to any of the companies would at least have to equal the cost of outbound cargoes, handling charges on the same, all shipping expenses met in Europe plus taxes, interest, and the value of all bills of exchange

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drawn on the company. From the standpoint of economic analysis, it must be remembered that no company had a monopsony (exclusive right of purchase) in India comparable to the Dutch position in the Spice Islands, but that each major company had a legal monopoly in its own home market; thus, the primary object of exploitation was the home buyer rather than the Indian supplier, even though much of the monopoly profits had to be shared with the home government through one form of taxation or another. No one today can answer these problems exactly, but a few caveats about some of the analytical problems involved would most likely have enhanced the value for the serious student of this otherwise excellent volume.

JACOB M. PRICE
University of Michigan, Ann Arbor

THOMAS A. METZGER. *Escape from Predicament: Neo-Confucianism and China's Evolving Political Culture.* (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1977. Pp. x, 303. $15.00.

This is an exceedingly rich and complex essay whose aim is to describe the ethos of Chinese civilization. In preparation, the author's reading took him into a dazzling range of sources, from the classics to Sung, Ming, and Ch'ing philosophers, and from there to modern scholars. In his own words, those moderns include Neo-Weberians, humanists (including philosophers), anthropologists, behaviorists, and intellectual historians.

To take on such an enormous task, Thomas A. Metzger develops his own methodology, and that in turn derives from his own analysis of the problem. He rejects existing descriptions of China's ethos as dependent and undynamic, arguing instead that an "ethos of interdependence" has characterized Chinese political culture since at least the later imperial era. In that ethos—which he defines as, and limits to, "the shared cultural orientations" of the educated elite—there was a traditional religious commitment to the traditional heritage itself. This commitment and its major formulations (such as those by Chu Hsi, Lu Chiu-yüan and Wang Yang-ming) are analyzed in the essay. He further shows that in modern times the "traditional commitment" has not perished, despite the fact that the former object of the commitment, the traditional heritage, has been greatly undermined and largely rejected. Modern philosophers, he shows, have tried to transform the traditional commitment by re-examining the heritage and demonstrating thereby its universal and timeless validity.

Metzger's own analysis of the heritage of Neo-Confucianism differs from those of modern Chinese philosophers. In his approach, he makes an effort to comprehend "the grammar of intellectual struggle." He employs, to the extent allowable when writing English, the actual terminology (in Romanized form) of intellectual argumentation. This device is a bit awkward from a stylistic standpoint. Each usage of a Chinese term (for example: *hsü-ling ming-chüeh*) is followed in parentheses by the English meaning ("intelligent awareness in its pure, naturally given, cosmically indivisible form, empty of any consciously specific concepts or sensations").

Despite its awkwardness, the method clearly yields valuable rewards. A cultural system's way of thought is embedded in its own "grammar" or vocabulary. Yet translation often leaves the actual words behind. Metzger's method manages to keep the vocabulary of Neo-Confucianism in the foreground. This is not to say he escapes entirely the translator's predicament, but he does take a giant step toward the understanding of Chinese thought through its own terminology and clichés (to use his own word). The glossary and terminological index provided by the author significantly enhances the book's value for further research.

The "predicament" of the title is not a word used by Chinese thinkers to describe their own ethos, as Metzger points out; it is Metzger's own label for a central *Problematik*, as he calls it, in Neo-Confucian thought. The predicament was the clash of two awarenesses: the awareness that one must, as a moral individual, as a human being, pursue the Neo-Confucian goal; and the awareness that there was a "massive tendency toward moral failure" on the part of humanity. In other words, "The Neo-Confucian predicament . . . was a paradoxical alienation from a constantly accessible truth which had to be pursued" (p. 160).

The author's analysis of the goals of Neo-Confucianism is very useful. A great deal of variation existed with respect to their formulation, yet there was an internal consistency among them as a whole. Metzger describes them as moral purity; sagehood; total cognitive clarity; coherence, control, and fixity; linkage, such as between heaven and humanity; cosmic and political power; and joy and social oneness.

Metzger argues that this "sense of predicament" was inherent in all the writings of Neo-Confucians in the later empire. The "predicament" was incapable of solution, yet no one could dismiss it as unworthy of thought and concern. Modern Chinese philosophers and humanists who have interpreted Neo-Confucianism have failed, in Metzger's view, to direct proper attention to this predicament. Its entire existence has been "filtered

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out of Neo-Confucianism by its great philosophical interpreters in our century" (p. 160).

Metzger has written an important essay. In addition to being an enlightening contribution to our knowledge of Neo-Confucian values, it is a challenge to modern interpreters of Neo-Confucianism. It will be interesting to see what responses it evokes.

JOHN D. LANGLOIS, JR.
Bowdoin College

C. A. CURWEN. *Taiping Rebel: The Deposition of Li Hsiu-ch'eng*. (Cambridge Studies in Chinese History, Literature, and Institutions.) New York: Cambridge University Press. 1977. Pp. vii, 357. $24.95.

Li Hsiu-ch'eng was the brilliant military commander whose vigorous and humane leadership helped prolong the days of the ill-starred Taiping Heavenly Kingdom. Li was captured by Tseng Kuo-fan in 1864, but during the eight days before his execution he was allowed to record his life history and experiences as a Taiping leader. The resulting deposition, fully eighty-four pages in translation, is one of the most precious survivals of the rebellion, for it is one of the few documents that records the motives and judgments of a participant on the *rebel* side.

Adding to the interest of this document is the fact that Li Hsiu-ch'eng became something of a popular hero in China, especially during the 1930s and 1940s. Yet in 1963–64, Li and his deposition paradoxically became the focus of one of the recurrent campaigns against revisionists in the People's Republic. Li Hsiu-ch'eng, it was now charged, had sold out to his captors and thus exemplified the capitulation of revisionists to class enemies. The saga of Li Hsiu-ch'eng is therefore of contemporary political, as well as historical, interest.

C. A. Curwen, lecturer at the University of London's School of Oriental and African Studies, has accomplished three things in this volume. First, he has written a long introduction, not only describing the history of the several editions of Li's deposition, but providing a richly informed discussion of many aspects of the Taiping movement that will be of value to Chinese historians who hold only a peripheral interest in the rebellion. Second, there is the translation, based upon the facsimile edition published in Taiwan in 1961. This translation scrupulously differentiates the original deposition from the altered version which Tseng Kuo-fan submitted to the Manchu court. (Curwen's discussion of Tseng's motives in altering the deposition is in itself a fascinating study of Ch'ing dynasty politics.) And, finally, the notes and annotations, half again as long as the translation itself, comprise a mine of historical and historiographical information that specialists on the Taiping rebellion will treasure.

This is a work of translation and annotation that meets the highest sinological standards.

LLOYD E. EASTMAN
University of Illinois, Urbana-Champaign

CHARLTON M. LEWIS. *Prologue to the Chinese Revolution: The Transformation of Ideas and Institutions in Hunan Province, 1891–1907*. (Harvard East Asian Monographs, number 70.) Cambridge, Mass.: East Asian Research Center, Harvard University; distributed by Harvard University Press. 1976. Pp. xii, 317. $15.00.

Hunan is in many ways a microcosm of China. It is also of major geopolitical importance. A commercial crossroads with a rich agricultural and mineral base, it developed an elite, marked by its education and affluence, which has played a major role in modern Chinese history. These are the opening themes of this important addition to the growing number of provincial studies of late Ch'ing China. Its central argument is that imperialist penetration was largely a function of changes within the province. Charlton M. Lewis concludes that the transformation of ideas and institutions in Hunan between 1891 and 1907 was brought about mainly by the orthodox Confucian literati who found room within the Confucian tradition for wide-ranging innovations. At the provincial level, he argues, the Western impact and the Chinese response dissolve into an intricate interplay of ideas as well as economic and social forces.

Lewis' evidence for the limitations on imperialism is convincing. His emphasis on the continuities between the Self-Strengthening movement and the late Ch'ing reforms counterpoints Mary Wright's view that Self-Strengthening represented the limit of Confucianism's flexibility. He is careful to balance change against continuity; for example, he sees his period as the point when growing regional power ceased to bolster and began to erode the imperial government.

Lewis also finds the last Ch'ing decade pivotal in the changing political and social roles of secret societies. The transformation of the literati into urban-focused modern intellectuals ended their traditional leadership role in the societies while economic modernization pushed the latter toward class revolution. But the organizational and ideological heritage of these groups made them ineffective and finally marginal. Lewis treats the societies

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as a primarily rural phenomenon, although this was not always true.

The author's discussion of late Ch'ing intellectual history raises a question. The intellectual processes which allowed the same orthodox Confucians who rejected Kang Yu-wei to embrace the late Ch'ing reforms are not described. Lewis argues that this was possible because Confucianism was adaptable, but one wonders if the literati did not increasingly substitute form for substance (like Confucian ceremonies in Western-style schools) because the philosophy no longer seemed functional.

Lewis believes that China's past offers little of relevance to the solution of her modern problems, but while this may be true it is still controversial. His perspective leads him to conclude that the search for a progress-oriented reinterpretation of their heritage by modern Chinese thinkers has been intellectually sterile and offers only psychological rewards. Is it possible, however, that modern reformers were concerned not only to justify Western-style reforms in traditional terms but also to fit them into a pre-existing reform pattern?

This is a stimulating book with new information and insights into some of the central issues of modern Chinese history.

H. MICHAEL METZGAR
North Carolina State University

CHARLOTTE FURTH, editor. *The Limits of Change: Essays on Conservative Alternatives in Republican China.* (Harvard East Asian Series, number 84.) Cambridge, Mass.: Harvard University Press. 1976. Pp. x, 426. $16.00

This book is a collection of essays by leading, though generally younger, scholars about the conservative side of China's modern experience. The essays cluster around the lives of ten Chinese leaders who dissented from some of the popular solutions to the problems of national salvation, solutions that in the process of adoption so effectively discredited the Confucian bases of the old order. They were contemptuous of the shallow utilization of Western ideas—including Marxism—as the foundation for the new order.

These conservatives, however, were modern men, not just mindless traditionalists. Shocked by Japan's defeat of China in 1895, shaken by the abolition of the examination system in 1905, and troubled by the unending problems of the Republican years, they were as outraged by Western imperialism and as committed to the Darwinian notion of the need to adapt in order to survive as were their liberal and radical rivals. Their life stories, so ably told in these essays, remind us that the currents of change in twentieth-century China cannot be neatly labeled conservative and radical. Indeed, the trichotomy of conservative/liberal/radical, a legacy of the Enlightenment, becomes an artificial construct not only for modern China, but perhaps for other times and places as well, as Benjamin Schwartz with characteristic freshness tells us in the opening essay.

Typically, these conservatives went through earlier radical periods of life prior to their critiques of indiscriminate Westernization. Some of the early ones, such as Chang Ping-lin and Liu Shih-p'ei, were themselves leaders in the 1911 Revolution and wielded great influence on the May Fourth leaders. After they had become disillusioned and turned conservative, their writings remained popular well into the Republican years; their names should become as familiar to students of Chinese history as Hu Shih and Ch'en Tu-hsiu. Even the arch villains (in some books)—Yuan Shih-k'ai and Chiang Kai-shek—appear here not as bad guys but as modern conservatives who command serious attention.

These essays suggest, furthermore, how "conservative" the revolutionaries were and still may be, notwithstanding vehement Maoist rejections of the idea. Core values in the Chinese Revolution echo the sentiments of the late Ch'ing champions of "national essence," the placing of public-mindedness, *kung*, above the private self, *ssu*, the stress on morality in politics and education, and the rural irritation with patterns of urban life associated with Western industrialization. *Limits of Change*, ironically, helps us to understand these characteristic Chinese elements of the revolution that elude dominant notions of social change and progress, both in Western thought and in Western scholarship. The book disabuses us of glib associations between "traditional" and "conservative," and between "modern" and "revolutionary."

The three essays of part four, "The New Confucianism of the Post May Fourth Era," stand out. That section of the book begins with Guy Alitto's excellent portrayal of the towering figure of Liang Shu-ming, whose similarity to the revolutionary Mao Tse-tung is quite revealing and ends with their differing purposes for the value of self-sacrifice—for Mao the nation-state and for Liang as an end in itself. The other two essays in this section are by Tu Wei-ming on the late Hsiung Shih-li and by Chang Hao on the "New Confucianism" among respected writers and teachers in Hong Kong and Taiwan. Tu's and Chang's essays convey the vitality of the conservative rejection of scientism (though not science) still current in Taiwan and China, while challenging the assumption that "modern life" and "Confucian humanism" are incompatible. Persuasively they criticize the value

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judgments of positivism and commitments to modernization that inform much of the current scholarly work on modern China.

PHILIP WEST
Indiana University, Bloomington

STANLEY WOLPERT. *A New History of India*. New York: Oxford University Press. 1977. Pp. xiii, 471. Cloth $15.95, paper $7.00.

This introductory survey is intended for the general reader and the beginning college student. Each is likely to find it at least as satisfactory as any other survey of the whole of South Asian history yet written. The writing is vivid, the causal explanations are sound and imaginative, and the narrative flow is maintained as in few other South Asian surveys.

The first quarter, on pre-Muslim India, is especially good. The author explains not merely what we know about ancient life but how we know it. He tells the reader about a piece of archeological, linguistic, or mythological evidence and then relates the speculative conclusion scholars have reached. For example, he suggests that when the god Agni's sacred fire consumes the forests in the *Satapatha Brahmana*, it is an allegorical reference to the Aryans' use of slash and burn techniques to clear the jungle during their migration eastward along the Ganges (p. 39). When men began to doubt the utility of the early Brahmans' costly sacrifices and ask questions about cosmic origins and design, Stanley Wolpert asks, "Was it the wastefulness of resources that appalled them most? The banality of time-worn ritual and uninspired mantras? Or the venality of those all-too-worldly priests who officiated? Perhaps it was the futility of it all, for where was the raja who never died? Or the rich man who never fell ill? Or the strong man powerful enough to survive the blow of a tiger's claw, or a mad elephant's tusk?" (p. 44).

The focus of the book shifts somewhat after the chapters on ancient India. The balance between religion, literature, mythology, art, and social life gives way to an emphasis on the political history and personalities of the rulers. Dynastic history provides the structure for brief discussions of simultaneous developments in cultural or economic areas. The author seems to be more interested in human personality and the complexity of motivation than in institutions. Because of Wolpert's gift for giving life to his characters, he maintains the high level of readability in the Muslim and subsequent sections.

In the last half of the book, which ends with the Janata victory over Indira Gandhi's Congress Party in 1977, Wolpert broadens his focus to include both nationalist leaders and the rulers. He incorporates the results of recent research on national politics, before and after independence. He also provides information about railway development, prices, taxes, imports and exports, and the position of women which is not available in previous surveys and which indicates in general some of the changes occurring in the wider society. The insights of recent studies of local and intermediate institutions and cultures are not, however, integrated into the analysis. Language reform, Hindi-Urdu conflict, vernacular literature, municipal politics, patron-client relations, rural entrepreneurial activities, counter-elites, marriage reform, and Sanskritization are either omitted or discussed briefly. Caste and agrarian change receive no sustained discussion in the chapters on modern India. Some readers may regret this, while others may welcome the continuity and the depth of the all-Indian political analysis made possible by this selectivity.

Teachers hoping to assign this book as a text in introductory courses may have one further regret. The chapters on modern India rarely inform the reader of conflicts in interpretation. The pedagogical value of this excellent book would have been enhanced by discussing scholarly debates.

The dozen maps are satisfactory. The lengthy, unannotated bibliography contains lists for further reading arranged not alphabetically but according to the author's priority. The priority rankings as well as some of the inclusions and omissions struck this reviewer as highly idiosyncratic. Teachers may wonder, for example, why a textbook and a biography by W. W. Hunter would be ranked first while books such as P. J. Marshall's *Problems of Empire: Britain and India, 1757–1813* and Sumit Sarkar's *The Swadeshi Movement in Bengal* are not even listed.

JOHN R. MCLANE
Northwestern University

JUDITH M. BROWN. *Gandhi and Civil Disobedience: The Mahatma in Indian Politics, 1928–34*. New York: Cambridge University Press. 1977. Pp. xix, 414. $32.50.

Although complete in itself, this history of the 1928–34 period is best understood in conjunction with the author's earlier *Gandhi's Rise to Power: Indian Politics 1915–1922*, to which the present work is a sequel (Gandhi's political career having been interrupted by his imprisonment in 1922). As did Gandhi himself in his autobiography covering an earlier period, Judith M. Brown attempts to show how the religious commitments of the man inevitably found expression through political involvement.

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She begins with an analysis of how the Mahatma, after his release from prison in 1925, took stock of the Indian political situation, the requirements for national leadership, and the weaknesses of the *raj*. Gandhi concluded that alien domination made impossible any rebirth of Indian vitality or self-respect, and that nonviolent action remained the only method of challenge consistent with Indian ideals or, under the circumstances, likely to prove effective. The salt *satyagraha*, with its dramatic two-month, 240-mile march across Gujarat to the Arabian Sea, was followed by Gandhi's mass civil disobedience campaign to force Lord Irwin to a conference table. These tactics succeeded in bringing about the 1931 London Round Table Conference, which the Mahatma attended from September to November. Failure to reach any major agreement resulted in the collapse of the civil disobedience movement and Gandhi's retirement from political leadership of the Congress Party. In 1933 "he was back in prison, leaving Congress saddled with a policy which its adherents neither desired nor understood. . . ." (p. 349). Thereafter his influence in India stemmed from moral example rather than political finesse.

Brown's account of Gandhi's aims, successes, and failures shows the extraordinary complexity of leadership in the subcontinent. She details the almost impossible task faced by politicians in attempting to establish "continental" as against merely provincial support in a nation divided by so many languages, castes, traditions, and geographic loyalties. Various regional figures in Bengal, Maharashtra, Gujarat, and elsewhere were available for action in the nationalist movement, but only Gandhi was able to propound policies and programs carrying appeal throughout India. The author effectively demonstrates the skill with which Gandhi reconciled competing communal interests and focused their power upon national issues.

In so doing, she has made use of a number of unpublished sources, including the V. S. S. Sastri and M. R. Jayakar papers. The letters and memos of Indian leaders and British officials depict a political Gandhi with living detail and color: his confidences shared with Andrews, Nehru, Jayakar; his tactics at critical meetings and conferences. The book includes ten tables and three appendixes listing statistics on Congress membership, salt consumption, elections, convictions for civil disobedience, etc. The one illustration, other than a 1931 photo of Gandhi, is David Low's cartoon in the *Evening Standard*, caricaturing Gandhi at the Round Table Conference. The cartoon alone is worth the price of the book.

The thorough scholarship, unbiased approach to controversial subject matter, and use of new sources make this work essential reading for the historian of modern India.

D. MACKENZIE BROWN
University of California, Santa Barbara

CHARNVIT KASETSIRI. *The Rise of Ayudhya: A History of Siam in the Fourteenth and Fifteenth Centuries*. (East Asian Historical Monographs.) New York: Oxford University Press. 1976. Pp. xii, 194. $20.75.

Charnvit Kasetsiri's book is unique in its use of historiography to illumine the early history of a Southeast Asian kingdom. Charnvit relates two traditions in Thai historical writing, *tamnan* and *phongsawadan*, to changes in Thai society and to changing perceptions of society. *Tamnan* is essentially Buddhist history with an emphasis on the history of the Thai as a Buddhist community led by religious figures: *rusi*, *chipakhao*, *khru-ba-achan*, and *phumibun*, one with merit. *Phongsawadan* is dynastic history, the rise and fall of monarchs. Its outlook is secular. *Tamnan* is the earlier school, *phongsawadan* appears much later, the result of the rise of Ayudhya with its powerful kings and their centralizing role in Thai political life. The early history of Thailand, Charnvit writes, cannot be understood without reference to both historical traditions.

The chapters on the early history of the Menam Basin prior to the formation of Ayudhya provide the best summary of the period currently available. Especially important is Charnvit's discussion of the appearance of the Thai in the Menam Basin. The myth of the Nanchao origins of the Thai people has been with us much too long; it is a relief to have it, at last, demolished.

An alliance between the ruling families of Suphanburi and Lopburi brought a new dynasty, that of Uthong, to power in the area of Ayodhya, forerunner of Ayudhya. Ayudhya prospered, moving into the vacuum created by the decline of Sukhothai and Angkor. Ayudhya was to become the first real center of Thai authority in mainland Southeast Asia and the originator of a new style of politics, with the end of the period of independent, politically fragmented *muang* (towns). Competition between the families of Uthong and Suphanburi dominated internal politics for two centuries, but internal struggles do not appear to have hampered Ayudhya in its expansion to the north and west. The importance of the *tamnan* tradition is again illustrated by King Trailok's skillful use of Buddhism in bringing Sukhothai into the Ayudhyan state.

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Based almost entirely on Thai language materials, this is a challenging, exciting study.

CONSTANCE M. WILSON
Northern Illinois University

KEITH SINCLAIR. *Walter Nash*. New York: Oxford University Press. 1976. Pp. vii, 439. $21.50.

Walter Nash (1882–1968) was one of New Zealand's greatest statesmen, and one of its few political figures who were well known overseas. Prime minister for three years, leader of the opposition for nine, minister of finance for fourteen, and a member of parliament for almost forty, he was knighted in 1963. Fortunately for his biographer, Nash was also a compulsive collector of documents relating to his life. Every memo, letter, and report—even scraps of paper—were stored in his study, and overflowed into his garage, which at the end of his life was packed full. Keith Sinclair has performed a monumental task in writing a long and carefully documented life of Nash. In addition to sifting through this mass of material, he has also used "oral history" by interviewing some of the leading political figures, some of whom have since died.

What emerges is a clear picture of a Christian humanitarian who helped mold the welfare state when Labour came to power in December 1935. Sinclair reveals Nash as a visionary not a doctrinaire socialist, and a bureaucrat par excellence. Born and brought up in England, Nash came from a very poor family, and his education was minimal. He was, however, a voracious reader with an unusually retentive memory and thus could hold his own with university graduates. He married and emigrated in 1909 to New Zealand, where he was not successful as a businessman. He soon joined the Labour Party, and Sinclair underscores Nash's realization that, while unions were particularly important to the party, Labour also needed to gain support outside the city. He first ran for parliament in 1919 but was not successful until 1929. Sinclair shows conclusively that Nash's excessive attention to detail led to procrastination in making decisions, and, for one who loved power, it was strange that he distrusted his own intuitive judgment. But Sinclair admits that there was a certain element of shrewdness in his indecision, because some problems solved themselves.

One of the great values of this biography is the fact that it is also a political and economic history of New Zealand from 1929 to 1968 because of Nash's intimate involvement in all aspects of New Zealand life. From 1935 on Nash was totally absorbed in bringing about the welfare state, and Sinclair underscores Nash's promotion of a department of marketing to stabilize and secure fair prices and mortgage relief, and his efforts to redistribute national income by an elaborate system of pensions and increased family allowances. As prime minister from 1957 to 1960 he was preoccupied with international affairs. Sinclair brings out Nash's concern for racial discrimination, the poverty of the Third World, and the problems of the Cold War. He was his own minister of foreign affairs.

Sinclair has great praise for but also some criticism of Nash. His energy was stupendous. He lacked consideration for his subordinates and distrusted people; other foibles are also noted. Many personal details appear, particularly the enormous love and support Nash received from his wife.

There are forty-five pages of tightly packed footnotes at the end of the book followed by an impressive bibliography. A selection of photographs and political cartoons enhance the book.

SAMUEL CLYDE MCCULLOCH
University of California, Irvine

UNITED STATES

DAVID M. POTTER. *Freedom and Its Limitations in American Life*. Edited by DON E. FEHRENBACHER. Stanford: Stanford University Press. 1976. Pp. xi, 89. $6.50.

Leaving aside documentary works, David M. Potter published three books during his lifetime. Since his death Don E. Fehrenbacher has completed and published for him three more books, which Potter left unfinished. In every case Fehrenbacher has given his services with characteristic modesty and scruple. Seldom if ever has an American historian been so well served by a colleague.

The present volume consists of the Commonwealth Fund Lectures in American History, which Potter delivered in 1963. The four lectures, Fehrenbacher tells us in a graceful introduction, survived only in a rough draft that lapsed sometimes into mere outline. (Perhaps that is why they were not included in the larger volume of Potter's essays, *History and American Society*, which Fehrenbacher brought out in 1973.) The text required substantial reworking, but the editor never obtrudes. If the hand is sometimes Fehrenbacher's, the voice is always Potter's.

Throughout the 1960s Potter was, in his own words, deeply concerned "about the basic soundness of our social ideals." Somehow those ideals had failed to withstand—had perhaps even engendered—anxiety, aggression, and alienation. The

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central conundrum for Potter was the ambiguous, complex relation between freedom and control. More and more, what seems to have emerged in his mind as the key to that relation was a distinctively American rejection of overt, personal authority. The first of the Commonwealth Lectures gives a brilliant interpretation of the American style of freedom as infused with repugnance for naked authority. "The basic idea in all cases was that there should be no lords—no lords spiritual, no lords temporal, and no lords economic" (p. 16).

The following lectures are thinner, less historically specific, more flatly dependent on the dated sociology of the 1950s. One of them points out the high price Americans have paid, in loneliness, isolation, and immaturity, for their escape from fixed status and acknowledged subordination. The last two lectures echo Riesman and Tocqueville in showing how the rejection of overt authority has made coercion subtle, impersonal, and manipulative. The argument is largely familiar. But Potter was moving onto new ground, still largely unexplored, as his earlier interest in the idea of equality was overtaken by a growing appreciation of changes in the structure of authority. The final somber essays in *History and American Society*, written in 1969 and 1970, suggest how fruitfully he might have developed that theme had he lived.

George Harmon Knoles has added to the present volume a valuable bibliography of Potter's writings, including the major reviews of his books. David Potter would have appreciated the elegaic note that sounds again in a publisher's notice on the back of the last page: "This is the last book set in metal, printed by letterpress, and bound in cloth by the skilled craftsmen of Stanford University Press 1895–1976 *Tempus edax rerum.*"

JOHN HIGHAM
Johns Hopkins University

Dictionary of American History. In seven volumes. Rev. ed. New York: Charles Scribner's Sons. 1976. Pp. ix, 502; 503; 504; 472; 472; 472; 419. $340.00.

Members of the American Historical Association are familiar with the late James Truslow Adams' five-volume *Dictionary of American History*, a formidable row of outsized books published by Charles Scribner's Sons in 1940, for which a supplementary volume appeared in 1961, and it must now be a matter of considerable professional interest that Scribner's has brought out a revised edition of seven volumes and will soon add an eighth, the index to the set. The revised edition constitutes a large publishing enterprise. Its publication doubtless has proved very expensive. The publisher has had to provide for an in-house staff—Louise Bilebof Ketz, managing editor, Joseph G. E. Hopkins, editorial adviser, Marshall de Bruhl who was director of reference editorial, and four copyeditors and five proofreaders. Honoraria have gone to the revised edition's editorial board of six scholars and to the approximately eight hundred new contributors to the revised edition. One presumes, however, that the return on this investment will be commensurate with the expense. The books are available not in bookstores but only from the publisher, and hence there is no discounting. At least 2,500 university and college libraries will purchase the set. Community colleges will buy it. This says nothing of the thousands upon thousands of public libraries and high school libraries. When the first edition came out, the then chief librarian of the Brooklyn Public Library and former president of the American Library Association, Milton James Ferguson, wrote J. T. Adams' managing editor, R. V. Coleman, that "I cannot picture any public or school library so small that the *Dictionary of American History* will not be indispensable." The same conclusion will be drawn today.

In accord with the scale of this large enterprise the publisher, Ketz writes in her foreword, made every effort to ensure the scholarly quality of the *Dictionary*. Scribners arranged for a far more broadly based board than obtained in 1940; in the earlier era the board consisted of seventeen members, twelve of them prominent historians; for the revised edition there was only one prominent historian, Thomas C. Cochran of the University of Pennsylvania, and the other five members represented military history, science and technology, political science, economics, and the arts: Harold W. Chase, University of Minnesota, Jacob E. Cooke of Lafayette College, Robert W. Daly of the Naval Academy, Wendell Garrett of the magazine *Antiques*, and Robert P. Multhauf of the Smithsonian. Robert F. Spencer of the University of Minnesota and Kenneth M. Stewart of the University of Arizona were specially commissioned to expand the coverage of the native American Indian and remove the bias of articles written during the 1930s, and Henry N. Drewry of Princeton did the same for the history of the Afro-American. The new board, Ketz relates, then set to work and "reviewed every entry in the first edition and the supplement and determined which of the articles could be retained after rechecking, editing, and updating; which articles needed to be revised completely, either because the entries in the first edition or supplement were unsatisfactory or because recent research indicated that a different approach was needed; and which entries were no longer important and could thus be deleted from the *Dictionary*."

The first edition contained 6,425 entries, from

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about 1,000 contributors. The size of articles went from 50 to 5,000 words. Entries for the revised edition total 7,200; 500 entries are new, and others were revised; there is a total, as mentioned, of 800 new authors. In the case of the first edition, according to Ketz, "The editors sought in every instance to assign a subject to the scholar most familiar with it." This principle, she relates, carried over into the revision.

It is saddening to write that the result of all this work, the revised edition, must be described as of questionable value in terms of scholarship—if financially it is destined to be a great success. The new board of editors should have insisted on virtually a clean sweep of the articles, cutting useless entries, updating those that survived, adding articles where scholarship and popular interest demanded. Instead they allowed for a weak revision, even though all the volumes were completely reset and everything could have been rewritten.

The opportunity was available to get new contributors, and the editors allowed it to pass, permitting the in-house editors to shove the first edition delinquents into the revision. Consider Ketz's statement that in 1940 the editors sought in every instance to assign a subject to the scholar most familiar with it. How explain the achievement of Alvin F. Harlow in the revision—for he is credited with 186 articles, from Adams Express Company to an article on Yorktown, including in his versatility accounts of the Danbury Hatters' Case, Drake's Oil Well, the Eads Bridge, Paving, the Pious Fund Controversy, Tar and Feathers, and Whiskey. His nearest competitor is another survivor from the first edition, Adams' associate editor, Thomas R. Hay, who in the revision is listed for 115 entries. They are almost entirely on military subjects, running the gamut from Allatoona Pass to an account of the battle of Winchester. There is an enormous amount of military history now being written and why let Hay's entries ride over into the revision? But Hay's credits for the revised edition do not include merely military subjects. He is the revision's author of Brannan Plan (in the index of contributors it appears as Brannon Plan). For the revision he also is credited with North Atlantic Treaty Organization and a companion article on Southeast Asia Treaty Organization. His entry for the latter bears signs of changes as late as 1972, but it incautiously looked forward to the mid-1970s: "SEATO remained in the mid-1970s a possible means of immediate opposition to any sudden revival of communist aggression in the region and exercised a stabilizing influence in the area." The date of Ketz's foreword is July 4, 1976, and the printing schedule for the *Dictionary*, especially for volume 6, must have given some chance to redo or patch up the SEATO entry. By the bicentennial of American independence SEATO was in a state of dissolution, and by the present writing it has dropped out of sight, though the treaty itself lingers on.

Everyone looks up his own specialty and those of his friends to see how the *Dictionary* handles them. The reviewer noted an article on the Kellogg-Briand Pact by B. H. Williams, a writer of a generation ago, that cites two books of the 1920s. Article 10 contains a bibliographical reference to the work of Denna F. Fleming, now entirely superseded, and no mention of the book by J. Chal Vinson. Mexican War is by J. M. Callahan and ignores the work of Norman A. Graebner and especially David M. Pletcher. Battle Fleet Cruise around the World, by Louis H. Bolander, makes no mention of the book by Robert A. Hart.

But the most serious problem with the *Dictionary* is coverage. Everyone will have his own ideas of what constitute important subjects in American history, and the board of editors doubtless had to make some arbitrary decisions. Perhaps a reviewer should list important subjects omitted. Still, the *Dictionary* should not consist of trivia, and it is here that the *Dictionary* is really open to criticism. In the way that most pieces of writing put their best foot forward in the opening pages, one might have expected the coverage at the beginning of the *Dictionary* to be better than in later sections, but if it represents the best of the volumes then the coverage of the others leaves much to be desired. The reviewer checked the first hundred pages of volume 1, all for the letter "A" (the letter A runs to p. 239); a hundred pages of A's seem sufficient for a detailed examination. In the first hundred pages are 129 entries, exclusive of cross-references. Of those 129 entries I question the reason for inclusion of nearly half, that is, of 60. The initial entry on the first page is Aachen, the first large German city taken by American troops during the Second World War. The battle lasted a few days and General J. Lawton Collins sent in a division to take the city. The German commander was a colonel—meaning that the Americans' opposition was not large. Does this battle deserve notice in the *Dictionary*? There follow entries on the ABC Conference, Abilene, Ab-Initio Movement, A. B. Plot, Abraham Lincoln (fort), Absentee Ownership, Acadian Coast, Accidents, Ackia (battle), Acoma (an Indian town about sixty miles west of the Rio Grande), *Adair* v. *United States*, Adams (fort; not the fort in Newport, R.I.), Administrative Discretion, Adobe Walls (battle), *Adventure* (first sailing vessel built on the Pacific coast), Adventurers (merchant adventurers), *Aeolus*.

One must pause in the recital of superfluous entries for the entire text of Frank Freidel's article on *Aeolus*: "One of the early experiments in rail-

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road cars, the yachtlike *Aeolus* was designed to sail before the wind. It was tried on the Baltimore and Ohio Railroad in 1830. On one occasion the *Aeolus* failed to stop when it reached the end of the finished track and ran into an embankment."

There follows an updating of Afro-American items: African-American Relations, Afro-American Church, Afro-American Culture, Afro-American Migration. The entries seem superfluous, considering that they precede a thirteen-column analysis of Afro-Americans by Henry N. Drewry.

The entries move on: Agamenticus (locality in colonial Maine), Agrarian Movement, Agriculture (American Indian agriculture, following a long entry on Agriculture in general), Aguayo Expedition (1720–22), Ague (shakes). Because technology now is being covered, the airplane has separate entries: Air Cavalry (helicopter gunships); Aircraft, Bomber; Aircraft, Fighter; Aircraft Carriers and Naval Aircraft. These entries are in addition to Aircraft, Development of. There also is Aircraft Armament; Air Defense; Air Force, United States; Air Power, Strategic. Aisne Defensive (First World War) appears, for no reason, for there is an entry for Belleau Wood and another for Chateau-Thierry Bridge; for this period of the fighting in 1918 there might well have been an entry for Vaux, a model operation compared to Belleau Wood. Aisne-Marne Operation appears; it should be only a crossreference, and refer to Soissons, Battle of (for which there is no entry, nor even a cross back to Aisne-Marne Operation). Other entries are Akron Law (an Ohio law of 1847), Alamance (battle of 1771 against the Regulators). There is an entry for Alaska-Pacific-Yukon Exposition, in which Frank Monaghan, a holdover from the first edition, relates that it was a minor but highly successful exposition; it was held in 1909. Entries appear for Alaskan Indians, and for Aleut (in addition to an article on Alaska). There follows Albany, and Albany convention (1689–90), *Albatross* (the Yankee-owned ship that brought news of the outbreak of the War of 1812 to W. P. Hunt, partner of the Pacific Fur Company, at its Astoria post), Alien Landholding, Aliens (rights of), Allatoona Pass (battle), Allegheny Mountains (routes across), Allegheny Portage Railway (a thirty-six-mile span between Hollidaysburg and Johnstown), *Alliance* (Continental frigate), "All Quiet along the Potomac," Alta California (it might have been better to make only a crossreference to California; incidentally there is an entry for Lower California but none, not even a crossreference, for Baja California), Alta Vela Claim (a masterpiece by the late Samuel Flagg Bemis), Alton Petroglyphs (an imaginary bird painted on a bluff near present-day Alton, Illinois, important for American Indian history), Ambassadors (the *Dictionary* has no entry nor crossreference for Ministers), Amen Corner (niche in the corridor of the old Fifth Avenue Hotel), *America* (nineteenth-century yacht), America (discovery and early exploration), America First Committee (there is no entry for the Committee to Defend America by Aiding the Allies), American Bottom (Mississippi River flood plain between Chester and Alton, Illinois).

Looking beyond the first hundred pages one espies other doubtful entries. American Expeditionary Forces is followed by American Expeditionary Forces in Italy, a strange category because there were only a few troops. Americanism is a discussion of the attitude of the Catholic Church at the turn of the present century. Ananias Club refers to Theodore Roosevelt's remark. Arbitration is about labor disputes. Arms Race with the Soviet Union seems a peculiar entry. Asia, Trade with, is an odd category. Balfour and Viviani-Joffre Missions is a holdover from the 1930s. Biddle Mission is an obscure affair of 1836. An entry appears for British Campaign of 1777 (other years?).

All this says nothing about wordage—decisions on the length of articles. Alaska has a very long entry by the late Senator Ernest Gruening, who wrote twelve columns; presumably it was awkward to cut Gruening's article down to size, even though Alabama has had at least as illustrious a history as Alaska and gets three columns. Baker, Bobby, Case has a half column, and Watergate has four. Black Capitalism has a full column, Black Cavalry in the West a full column, Black Infantry in the West a full column. Johns Hopkins University has four columns, and there is no entry for University of Wisconsin, University of Michigan, Indiana University, University of California, etc. etc. Korea, War with (1871) receives a full column for a conflict resulting in three Americans dead and nine wounded; Korean War (1950–53) has four columns.

The reviewer ended his search by looking up the article on "Turkeys, Wild," wherein it is written that "their wide distribution and numbers made them an important article of food for travelers, pioneers, and later for soldiers . . . The unwillingness of the turkey to take to its wings, and its preference for hiding, after a short flight, . . . made it a comparatively easy prey . . . Some were even run down by horse and dog on the prairies. . . . [They] often weighed 25 to 30 pounds . . ."

ROBERT H. FERRELL
Indiana University, Bloomington

WILLIAM H. CHAFE. *Women and Equality: Changing Patterns in American Culture*. New York: Oxford University Press. 1977. Pp. xiii, 207. $8.95.

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What is women's history? Can a group whose experience crosses national, class, and ethnic lines realistically be studied as a separate entity? It is these questions and others related to the structure and methodology of women's history that William H. Chafe attempts to confront in this volume. Essentially the book is a series of essays in which historical analysis, sociological theory, and the author's own predilections are brought to bear to explain the spectrum of women's experience since the Colonial period, the rise of feminism in the 1970s, the future potential for feminism in America, and the extent to which all these developments can be related to any special women's culture. Chafe's conclusions in all of these areas are sufficiently qualified that it is difficult to take exception to any of his generalizations. For instance, he argues, regarding the controversial question of the relationship between industrialization and nineteenth-century feminism, that industrialization provided the "context" for that feminism, whose roots lay in religion, the reform ferment of the 1830s, and the new higher education for women. In tracing the origins of the women's movement of the 1960s, he thinks that the Second World War was key, although he strongly qualifies this generalization by criticizing those historians who attribute change exclusively to impersonal forces.

Nonspecialists in women's history will find Chafe's historical sections a good introduction to the field; for specialists they are too brief to be definitive and, despite the use of sociological theory, offer no strikingly new interpretations. His analysis of the future of feminism, however, is an exception. Chafe here effectively delineates the strength of contemporary feminism in comparison with its historical forerunners because of its relevance to a society in which most married women work, because of its emphasis on a variety of issues, and because of its decentralized power base—attributes not characteristic of earlier feminist movements. Chafe might also have mentioned modern feminism's firm organizational underpinnings, not only in political and activist groups like the Women's Political Caucus, but also in professional caucuses (like the Central Coordinating Committee for Women in the Historical Profession) which now exist in many professions.

This book is most challenging in the two essays which compare the situation and recent history of women in the United States with that of blacks. The use of this comparison as an analytic tool is not new, but never before has its potential been so fully explored. Although Chafe makes clear that the oppression of blacks and their consequent resistance has been much greater than that of women, he cogently argues that the means by which blacks and women have been rendered subservient have been similar. In addition, he argues that the development of black activism and women's activism in the 1960s had important similarities. As Chafe makes clear, the usefulness of the analogy lies not only in the light it sheds on the experience of each group but also in what it reveals about the nature of both social control and collective protest within the United States. It also points to the general utility of comparative history and to the provincialism of American historians, who generally study one group and one nation in isolation.

In the final analysis, like most writers who theorize about women's history, Chafe arrives at a conundrum. For, as he concludes, it is difficult to find another minority group whose members differ so completely in their own perception of their status and who live so constantly in contact with their presumed oppressors. In fact, women have a number of "cultures" or constellations of shared experiences which are uniquely feminine in nature. One, as Chafe points out, is that complex of domesticity, childbirth, and childrearing whose parameters women themselves have consistently defined. A second, which Chafe overlooks, is the culture of fashion and beauty which, since the nineteenth century, has been uniquely applied to women and which, despite its general treatment by historians as frivolous, has been American society's most effective agent in dividing and oppressing women.

Though we may disagree with Chafe's conclusions, the underlying principle of this work is irrefutable: that women's experience can be treated as a separate entity and that it can be compared with the experience of other groups within the culture. Though we may argue about the structure of women's history, it is by now indisputable that there is such a field.

LOIS W. BANNER
Rutgers University, New Brunswick

GLENN DAVIS. *Childhood and History in America*. New York: Psychohistory Press. 1976. Pp. 281. Cloth $12.95, paper $5.95.

It is a truism that the children of today are the parents and citizens of tomorrow. Most of us take it for granted that the way children are treated by their parents and by society at large influences their adult behavior and hence affects the course of history. Glenn Davis is not content with such generalities. A convert to the psychogenic theory of history formulated by Lloyd deMause, Davis (paraphrasing deMause) asserts "the evolution of

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society within the family is the root *cause* of the evolution of society" (p. 14) and "the genes of historical change lie wholly in adult-child interactions" (p. 20).

Davis devotes the first part of his book to an exposition of the psychogenic theory, which he regards as "not an invention but a discovery" (p. 27). Part of the hypothesis (as most students would call it) is that parent-child relationships in the West have passed through six modes: infanticidal (antiquity to fourth century A.D.), abandonment (fourth to thirteenth century), ambivalent (fourteenth to seventeenth century), intrusive (eighteenth century), socializing (nineteenth century to mid-twentieth), helping (beginning mid-twentieth century). Davis, applying rather than testing the theory, devotes part 2 to the evolution of American childhood from 1840 to 1965. By "placing deMause's socializing mode under a microscope" (p. 27), he identifies four sub-modes or patterns of parent-child relationships: psychic control (1840–80), aggressive training (1880–1910), vigorous guidance (1910–40), and delegated release (1940–65). Examining sources such as diaries, child-rearing manuals, autobiographies, and fiction, he traces progress from dominance toward permissiveness. Part 3, dealing with the periodization of twentieth-century American history, presents Progressivism, the New Deal, and the Great Society as adult expressions of individuals developed respectively under the psychic control, aggressive training, and vigorous guidance modes; he sees the youth movement of the late 1960s as a manifestation of the delegated release psychoclass.

As Joseph Kett pointed out in a review of deMause's *History of Childhood* (*AHR*, 80 [1975]: 1296), the psychogenic theory ignores "the cultural, economic, and demographic constituents of any given society's treatment of children." The theory seems designed to limit the history of childhood to questions psychoanalysts and psychoanalysts-turned-historians feel pre-eminently qualified to examine. Without denigrating the usefulness of psychohistory, I believe the history of childhood embraces not only parent-child relationships, but many other factors including public policy, external forces such as war and depression, and peer-group relationship.

In part 2, especially "Psychic Control, 1840–80," the most substantial chapter in the book, Davis treats a wide range of sources relating to the history of childhood in mid-nineteenth-century America. This chapter will interest students of the role of social control in social reform. Davis' treatment, however, is too hurried and superficial to be either informative or convincing. He seems less interested in understanding the history of children than in demonstrating the rectitude of his mentor's teaching. The book is characterized by a strident tone more appropriate to the huckstering of a "magic-formula hair restorer" than to serious historical study.

ROBERT H. BREMNER
Ohio State University

LAWRENCE A. CREMIN. *Traditions of American Education.* (Merle Curti Lectures, 1976.) New York: Basic Books. 1977. Pp. ix, 172. $8.95.

Traditions of American Education comprises three Merle Curti Lectures which Lawrence A. Cremin delivered at the University of Wisconsin in March 1976. Essentially the lectures summarize Cremin's work to date on his three-volume comprehensive history of American education.

Cremin directs his attention to the full range of social institutions and to the different ways that individuals and groups have experienced those institutions at various periods in American history. He underscores the educational role of such important institutions as families, peer groups, churches and synagogues, museums and libraries, settlement houses, factories and business corporations, publishers, radio and television networks. Such a broad view of public education encompasses all the institutions that educate the people, whether or not they are part of the formal educational establishment. Thus Cremin expands the whole notion of public education and emphasizes that it occurs through families, churches, and television stations as often as it does in schools.

Cremin's work is an example of a revisionist thrust that has marked the historiography of American education during the past two decades. Cremin defines education as "the deliberate, systematic, and sustained effort to transmit, evoke, or acquire knowledge, attitudes, values, skills, or sensibilities, as well as any outcomes of that effort." His definition extends beyond the schools and universities to the variety of individuals and institutions that educate the people. Such a conception runs counter to modern educational thought which focuses mainly on the role of the school as a social institution.

The effort to broaden the history of education to include more than schooling is a worthwhile corrective to earlier school-oriented histories. We should not, however, be misled into underestimating the role that public schools played in the nineteenth and early twentieth centuries. Nor should we downgrade the idea of public schooling in the past or the present. Cremin contends that he is interested in making the public schools more effective and urges teachers and educational leaders to examine more critically both content and method.

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What emerges from Cremin's clearly written work is a rich, broad, and profound synthesis reflecting much hope and optimism. "Contrary to the drift of a good deal of scholarly opinion during the past ten years," states Cremin, "I happen to believe that on balance the American educational system has contributed significantly to the advancement of liberty, equality, and fraternity. . . . The aspirations of American education have been more noble than base. . . . Its performance over the past two centuries has been more liberating of a greater diversity of human energies and potentialities than has been the case in most other eras and in most other places" (p. 127).

S. ALEXANDER RIPPA
University of Vermont

STEPHANIE GRAUMAN WOLF. *Urban Village: Population, Community, and Family Structure in Germantown, Pennsylvania, 1683–1800*. Princeton: Princeton University Press. 1977. Pp. xi, 361. $19.50.

It has been more than a decade now since Page Smith drew the distinction between covenanted and cumulative communities, and more than a decade, therefore, that colonial historians have evaded its implicit challenge to probe the cumulative communities. Like Smith, they have devoted themselves instead to the covenanted communities of New England, with an assiduousness which defies justification if not explanation. For the ready accessibility of the New England towns is also their singularity. The social and cultural coherences that make them visible made them less material to the future.

The social pattern of the nineteenth century was prefigured far more profoundly in the associational settlements west of the Hudson than in the communitarian ones of the Puritans. And the heterogeneity, fluidity, and unconcern for encompassingly shared values of our own time were already evident in the casually clustered populations of the middle and southern colonies.

Urban Village is the first extended effort in the colonial literature to make sense of these straggling settlements, and, as such, it is a pioneering American community study. But it is also a richly rewarding and splendidly suggestive account of colonial life in its own right.

In dozens of ingenious and arresting ways, Stephanie Wolf exposes Germantown as an unstable society of transient and isolated individuals oriented as much to Philadelphia and the hinterland as to their own immediate environs and more to the market and the main chance than to any particular place. Shrewdly and scrupulously analyzing early land distributions, civic symbolism, and economic geography, she reveals a village that was essentially urban, in spirit and social organization, from its founding. Imaginatively canvassing mobility, occupational proliferation, family dispersion, and much more, she delineates the development of a town that became ever more fragmented and ever more attached to the cash nexus over the succeeding century.

In computations based on clever and often immensely inventive conceptualizations, Wolf shows us a mass of men and women who exhibited few peasant propensities and almost no traditional ties to land, family, faith, or community. And in illustrations based on acute and often resourceful readings of the texts, she illuminates these general findings with telling effect. Her account of the Americanization of the Germans is the most subtle and compelling study of the acculturation of a European population in the entire colonial canon. Her depiction of the confinement of familial obligation within the nuclear family and then to the callous and utilitarian calculations of its individual members is among the most provocative of our apprehensions of the colonial family.

And more than all that, *Urban Village* provides a vivid sense of the lives of its people. Beyond its methodological creativity and its conceptual coherence, it touches always the pulse of daily dilemmas and desires. Wolf summons her sources, German as well as English, with absolute mastery, and she has the assurance to transcend her tabulations and command her quantitative materials so as to achieve a surer feel for the life from which they took their rise. In her intimately detailed discovery of that life she has given us a convincing and satisfying portrait of a particular community and a stimulating interpretation of American community life that will be studied profitably for years to come.

MICHAEL ZUCKERMAN
University of Pennsylvania

WILLIAM B. WILLCOX, editor. *The Papers of Benjamin Franklin*. Volume 20, *January 1 through December 31, 1773*. New Haven: Yale University Press. 1976. Pp. xxxviii, 624. $20.00.

Editing the Franklin Papers is a monumental task. The material is so diverse and touches on so many topics that it is almost impossible to cover all of them thoroughly. There are the inevitable letters and papers, both to and from Franklin, on a variety of scientific topics and each of these must be put in context. Moreover, Franklin's role as continental spokesman during this period involved him in virtually every political controversy that arose between Great Britain and the several colonies,

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and here again the editors are called upon to provide background information. In this volume, for example, there is extensive comment on such issues as the constitutional debate between Governor Hutchinson and the Massachusetts General Court, the political situation in Great Britain, the passage of the Tea Act, Virginia's call for appointing committees of correspondence, speculation in western lands, and Franklin's role in forwarding the Hutchinson-Oliver letters (the texts of which are reprinted in an appendix) to friends in Massachusetts.

For the most part this volume is an excellent addition to an outstanding editorial project. The notes are concise and the text is not excessively annotated. Moreover, the index, perhaps the most important feature of such a volume, is outstanding. The editors occasionally fail to list every reference under all applicable subheads, but a number of random checks failed to turn up a single reference that was not noted under the general topic. The decision to limit entries under the heading of *Franklin* eliminates an unnecessary index topic which was unusable in previous volumes.

Unfortunately, however, there are occasional interpretative comments, in both headnotes and footnotes, which are out of place and sometimes misleading. In commenting on one of Franklin's letters about the Tea Act (pp. 226–28) the editors implicitly criticize Franklin for suggesting that the purpose of the statute was to "keep up the Exercise of the Right" to tax the colonies. They further suggest that Franklin's purpose was to show "a red rag to the Bostonian bull." The letter, read in context, will not bear that interpretation, and the criticism of Franklin is pure conjecture.

The decision to print resumés of certain Franklin letters is also unjustified. There are five such letters in this volume, all to Alexander Colden and all insignificant. But the space saved is minimal and the editors have involved themselves in a selection process which might better be avoided by simply printing Franklin's own letters in full.

DAVID AMMERMAN
Florida State University

CATHERINE L. ALBANESE. *Sons of the Fathers: The Civil Religion of the American Revolution.* Philadelphia: Temple University Press. 1976. Pp. xiv, 274.

Catherine Albanese has fashioned a graceful though heavily weighted craft which navigates a disputed passage with convincing success. With unobtrusive sophistication she has utilized the methodology and categories of "the history of religions 'form-comparative' approach" to present the American Revolution as "*in itself* a religious experience," a grand revival, with sacred symbols and spontaneous rituals, carried forward by the ardor and zeal of enthusiastic religion. Her analysis of "the religion of the American Revolution" is fresh, incisive, and convincing. It is fresh and convincing partly because she has engaged in a wide-ranging pursuit of new data. It is incisive and convincing because she has brought to the task a sensitive awareness of "how consciousness is structured, how symbols generate societies and how societies generate symbols, how a myth comes to be a myth and how it is transformed even as it transforms."

While the author's depiction of the fact and the general character of the religion of the Revolution cannot be faulted, greater clarity would have been achieved had she extended her analysis to include its seventeenth-century antecedents. She does report that members of the Revolutionary generation acknowledged themselves to be standing in direct succession to Oliver Cromwell and those who supported him. This clue should have explained to one who recalled "enclosures" and "ship money" why "liberty and property" (a phrase assiduously propagated in America by William Penn, among others) were indissolubly linked. There was no liberty for those who could be deprived of shelter and means of livelihood or reduced to penury by arbitrary levies without their consent.

Moreover, to one familiar with the pamphlet literature of Cromwell's time, it would have been clear that the main features of the religion of the American Revolution were deeply embedded in the colonial mind, having been elaborated during the English Civil Wars. A recognition of this fact would have saved the author from an undue emphasis on the Enlightenment, positing the notion of two Gods—Jehovah God and Nature's God. To most colonists, with Ethan Allen a notable exception, there was only one God, the creator and disposer of all events, who makes himself known in two differing ways or forms of revelation. Natural revelation was general revelation, an understanding of the moral law available to natural reason through the lessons of history. Natural religion had only an incidental relationship to "sermons in stones" and Newtonian concepts.

The artificial dichotomy between two Gods leads to some strange conclusions: that the two Gods were conflated during the American Revolution, that Jehovah God "got lost in the fray," and that it was his "colleague," Nature's God, who continued after the war to preside over the nation. The "Battle Hymn of the Republic" should be a sufficient reminder that Jehovah God had a considerable afterlife. Equally curious is the comment that "Jehovah had stopped making history, while the nation of patriots was obligingly making it for

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him." The stress by the colonists upon the appropriate use of means was no necessary denial of divine sovereignty and intervention. The doctrine of means, the utilization of human agents, was spelled out in Calvin's *Institutes* and was standard fare in most sermons.

While there was novelty and innovation in the religion of the American Revolution which gave it a new dimension, discontinuity with the past was not as great as *Sons of the Fathers* tends to suggest. This does not diminish, however, either the importance of the contribution the author has made or the freshness of her insight. Her book stands in the front rank of works dealing with this much debated subject.

WINTHROP S. HUDSON
University of Rochester

MERRILL D. PETERSON. *Adams and Jefferson: A Revolutionary Dialogue.* (Mercer University Lamar Memorial Lectures, number 19.) Athens: University of Georgia Press. 1976. Pp. xiv, 146. $7.00.

EDMUND S. MORGAN. *The Meaning of Independence: John Adams, George Washington, Thomas Jefferson.* Charlottesville: University Press of Virginia. 1976. Pp. 85. $7.50.

The principals of the American Revolution continue to act as a magnet, attracting the wisest and most learned scholars as they pursue this remarkable but somewhat elusive generation of Americans. These two slim volumes, each a series of essays based on lectures, prove once again that John Adams, Thomas Jefferson, and George Washington are inexhaustible subjects for investigation and synthesis.

The focus of each set of essays differs, but the conclusions tend to be reinforcing. Morgan concentrates on the dominant personal traits of Adams, Washington, and Jefferson and how the revolution enabled their individual characteristics to flower; Merrill Peterson is concerned with the exchange of ideas between Adams and Jefferson from their initial contact until death. (It should be noted that Peterson's essays are not an enriched version of Lester Cappon's *Adams-Jefferson* letters but rather the fruition of his own vast learning.) Morgan strictly limits the context of historical events; Peterson follows the unfolding of historical forces and provides a broader intellectual context with essays on "The American Revolution," "The French Revolution," and "The Revolution of 1800." In both books, however, men, rather than the flow of history, dominate.

Adams was freed from provincialism by independence, Morgan asserts, and he distinguishes between Adams' vanity and ambition. Fame was something sought by Adams, and independence offered him the opportunity. Franklin and Jefferson would have gained "large stature" regardless of time or place, concludes Morgan, but not Adams. It was independence that gave Adams a new and fertile arena for his talents; and in the first Continental Congress, asserts Morgan, Adams found "a field of action commensurate with his ambition" (p. 16). Not only did Adams instantly perceive the full consequences of independence, but he also welcomed them. His firmness in preserving its results rather than courting party favor enabled the second president of the United States to crush his vanity but satisfy his ambition.

Morgan is not blind to Jefferson's flaws, especially as they affected his attitude toward blacks and women. Yet Morgan gives Jefferson high marks. Adams stressed government and its institutions, states Morgan, but for Jefferson "the individual came first, governments and nations a poor second" (p. 69). This characteristic, asserts Morgan, sets Jefferson apart from other political thinkers, because he felt free from the imprisonment of inherited institutions.

Morgan's essay on Washington reflects the recent research of a number of scholars. Washington, he states, valued honor and "interest." Independence permitted him to couple these values with the struggle for independence and the creation of a new nation.

Peterson tells us that the friendship between Jefferson and Adams—except for the period of a dozen years after Adams left the presidency—was a triumph of seeming incompatibilities in personal temperament and intellectual style. Adams was conscientious, impulsive, voluble, sometimes awkward in social intercourse, stern, often satiric, at times filled with self-pity, insecure, and wary of innovation and the dangers of humanity's self-love. Peterson finds Jefferson reserved, careful, self-possessed, generally agreeable, sanguine about the future, possessed of all the graces for agreeable social intercourse, and generally animated by idealism and a belief in a leveling spirit which was not always exercised in practice. In contrast to Adams, he was a politician who could forget politics.

But Peterson detects a difference in the fundamental outlook of Adams and Jefferson on the causal factors of the movement toward independence. He believes that the Massachusetts Court Party was the principal culprit in Adams' eyes but that for Jefferson the revolt came wholly from the actions and folly of Great Britain.

Although Morgan and Peterson do not always use the identical set of adjectives, their assessments are remarkably similar. Peterson and Morgan also

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agree that, with independence won, Adams considered the Revolution complete but that Jefferson perceived the winning of independence as only the beginning. Peterson and Morgan share a conviction that Adams' search for fame served as the vital force that gave his personality and career its major thrust. Both authors stress that Adams became personally involved while Jefferson held himself in reserve, that Adams' explosive emotional responses were always visible, but that Jefferson remained the private person who continues to defy the most penetrating investigations.

I suspect that some scholars will query whether, in either of these volumes, Adams receives his due and whether Jefferson tends to be allowed the benefit of the doubt. The current resurgence of Adams in the public mind may well reflect an increased concern for integrity after several decades of shoddy, if not downright dishonorable, recent political history—a theme that could elevate Adams to a position almost equal to the genius at Monticello.

CLARENCE L. VER STEEG
Northwestern University

MERRILL JENSEN and ROBERT A. BECKER, editors. *The Documentary History of the First Federal Elections, 1788–1790*. Volume 1. Madison: University of Wisconsin Press. 1976. Pp. xxxi, 896. $30.00.

Scholars will welcome the publication of the first of three projected collections of documents pertaining to the nation's initial elections in 1788–90 for the House, Senate, presidential electors, and president. Richly informative and superbly edited, the current volume briefly covers the Confederation era and then focuses on the election processes in South Carolina, Pennsylvania, Massachusetts, and New Hampshire. Its extensive assemblage of pertinent correspondence, newspaper commentary, legislative proceedings, and election data will make the volume an indispensable reference.

Jensen and Becker deserve praise for their editorial work. Their purpose is not to write a history of the first federal elections, but to select, assemble, and place the documents in their historical context; in this they succeed admirably. The primary observation which emerges from the sources is the continuity between the debate over the Constitution and the first federal elections. Contemporaries viewed the contests as crucial to the fate of the nation and the implementation of the Constitution. Individuals as diverse and dispersed as Charles Pinckney and Theodore Sedgwick concerned themselves with questions involving the role of the people in government, the proper method for electing representatives, and other issues as yet unresolved by the Constitution. Oftentimes the controversy of 1787–88 continued unabated as former Federalists and Antifederalists sought to prevail in the elections in order to protest or to correct the newly ratified Constitution.

Numerous other observations emerge from the documents, only a few of which space permits noting here. Campaign exchanges were made on a relatively high plane, and there appears to have been little coordinated partisan activity within and especially between states. This possibly helps to account for the general dearth of antiparty expressions among a generation usually noted for its antipathy toward party. At times the nature of the sources, for example, the incompleteness of documents for New Hampshire and South Carolina, leaves certain questions unanswered. Despite such imperfections in the sources themselves, scholars are indebted to Jensen and Becker for their study. It is only hoped that the subsequent volumes will be as useful and as informative as the first.

PAUL W. BREWER
University of Maryland, University College

LINDA GRANT DE PAUW *et al.*, editors. *House of Representatives Journal*. (Documentary History of the First Federal Congress of the United States of America, volume 3.) Baltimore: Johns Hopkins University Press. 1977. Pp. xxix, 900. $28.50.

All who have strained their eyes reading the fine print of the 1826 edition of the *House Journal*, or cursed its miserable index, will cheer the appearance of this volume. Linda De Pauw and her associates have provided not only a reliable text, but also full cross-references to related proceedings and Senate documents. The masterpiece of an index makes it possible to find what one needs quickly and without the lingering fear that an essential reference might be missed. What problems remain are the fault of John Beckley, clerk of the House, who discarded many "unneeded" papers. He also ignored proceedings in the committee of the whole, making it impossible to follow bills as they were amended.

As the editor concedes in her preface, this is a very dull book. The journal tells only the official actions of the House and does not provide any account of the debates. It is, however, the essential guide to those debates, which are often misleading to those unaware of the exact circumstances of a proposed amendment or of a motion to refer a report. With confidence a reviewer can say that this edition is definitive. Would that it were pos-

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sible to undertake comparable editions for succeeding Congresses.

Although few historians will ever attempt to read this marvelous work of reference, those who browse through its pages will discover unexpected gems. All students of American history know of the long dispute over the location of the national capital or the intense argument over Hamilton's funding system. Very few know of the flurry of petitions to Congress seeking relief for widows of soldiers or wounded veterans of the Revolutionary War or the applications for copyrights from such authors as Jedidiah Morse for his *American Geography*. John Fitch petitioned for an exclusive right to exploit his discovery of steam navigation, and one William Hoy claimed that he had found "an infallible cure for the bite of a mad dog" for which he asked compensation, upon payment of which he would make public his discovery. Constituents frequently asked their representatives to help solve their problems in dealing with government bureaucrats, or to save them from the penalties of a strict enforcement of the revenue laws.

Change comes very slowly to Congress, and there is much here that would be familiar today on Capitol Hill. Those who seek to understand the legislative branch must do more than read accounts of floor debates. For the first Congress there is now a fully reliable guide with which to begin.

PATRICK J. FURLONG
Indiana University, South Bend

ROBERT A. RUTLAND *et al.* *The Papers of James Madison*. Volume 10, *27 May 1787–3 March 1788*. Chicago: University of Chicago Press. 1977. Pp. xxvi, 572. $25.00.

This volume of *The Papers of James Madison* covers the period from the opening of the Federal Convention through the completion of Madison's contribution to *The Federalist Papers* in March 1788. The multi-faceted contribution of Madison to the drafting and adoption of the Constitution is the pre-eminent theme of the documents and the regrettably few editorial notes printed herein. The volume illuminates Madison's role in the Federal Convention while the editorial notes make clear that Madison, like Hamilton, did not succeed in creating the Constitution he desired—one with proportional representation in both houses of the legislature and a central government vested with an absolute veto of state laws. The correspondence in this volume illustrates Madison's role as private promoter of the Constitution through a network of correspondents from Massachusetts to the Carolinas. Finally, the volume prints Madison's contributions to *The Federalist Papers*, headed by an editorial note that should settle the continuing debate over the disputed authorship of various of those papers.

While this volume represents a major accomplishment, there are certain gaps. The editors, for example, publish only a portion of Madison's speeches in the Federal Convention, noting that at some point in the future a new edition of the entire debates will be published either within *The Papers* or as an entirely separate editorial enterprise. Yet, such a series will duplicate the speeches printed in volume 10. If it is not published, the absence of those speeches will make *The Papers* less than definitive.

In a similar matter, the editors chose not to publish Madison's remarks in the Confederation Congress on September 26 and 27, 1787, when the report of the Federal Convention was before Congress. Granted the notes are not Madison's, but those of Melancton Smith of New York. Nonetheless, Congress's decision to forward the Constitution to the states was an integral part of the making and adoption of the Constitution. Madison's remarks then are important enough to be included or at least editorially noted.

These considerations notwithstanding, the editors of this volume have provided an outstanding collection for use by anyone interested in Madison and have rendered the task of research significantly easier for those seeking to understand the process of the writing and adoption of the United States Constitution.

STEVEN R. BOYD
University of Texas, San Antonio

HAROLD C. SYRETT, editor. *The Papers of Alexander Hamilton*. Volume 24, *November 1799–June 1800*. New York: Columbia University Press. 1976. Pp. xii, 708. $20.00.

For Hamilton fans this is a rather disillusioning volume. Not that there is any deterioration in the skilled editorial work of Harold Syrett and his associates. Indeed, their judgment seems excellent in avoiding the temptation to print everything Hamilton wrote; mercifully they relegate to a summary appendix the more routine and trivial communications of these months. Rather, our hero is mired in a swamp inhabited by petty, backbiting lesser officers, mindless bureaucrats, incompetent underlings, and a mutually despised superior. When he is not firing off letters trying to smooth over problems created by the drunks, embezzlers, and horse-thieves who wear a uniform in the army he commands, Hamilton is busy juggling officer

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rankings, begging approval of overblown expense accounts, or advising the president not to grant his son-in-law's request for a cushy post. Some of the juggled officers turn out to be dead; gambling, drinking, thieving, and whoring go on apace; and the padded expenses are never settled to anyone's satisfaction. Worst of all, Hamilton never seems to tire of dealing with such matters; nothing is assigned to subordinates. Why did a Founding Father, a political theorist of the highest order, the creator and practitioner of a great American tradition of energetic government, sink to wasting his talents so for months on end? In fact, General Hamilton often dreamed up exotic ways to consume time, paper, and money. Take the experiments he ordered on March 18 to determine the ideal number and length of steps per minute for drilling soldiers: short men, tall ones, thin, fat; with and without rifles, bayonets, backpacks; over hills, mountains, flatlands; in mud, rocky soils, snow—hundreds of men across the country marching around in circles for months while idle commanders procured appropriate pendulums. Results trickled in: by April 3 General Pinckney had made many experiments and planned many more; Colonel Rice dutifully reported the same 30 ¼ inch step for men 6 feet or 5 feet 6 inches tall but longer steps for men of intermediate heights and promised continued experiments. By the time Colonel Hamtramck responded from Pittsburgh Hamilton's army had been disbanded and the former general turned from military science to politics.

To Hamilton's highly partisan May 4th letter to Theodore Sedgwick, the editors append a thorough summary of major political currents in each state for the upcoming presidential election. Against this backdrop the last quarter of the volume establishes firmly where Hamilton stood. "I will never more be responsible for him [John Adams] by my direct support—even though the consequence should be the election of *Jefferson*. If we must have an *enemy* at the head of Government, let it be one whom we can oppose & for whom we are not responsible, who will not involve our party in the disgrace of his foolish and bad measures. Under *Adams* as under *Jefferson* the government will sink" (May 10, 1800).

RUDOLPH M. BELL
Rutgers University, New Brunswick

R. HALLIBURTON, JR. *Red over Black: Black Slavery among the Cherokee Indians*. (Contributions in Afro-American and African Studies, number 27.) Westport, Conn.: Greenwood Press. 1977. Pp. x, 218. $15.95.

At the turn of the nineteenth century, when they became "civilized," the Cherokee Indians got into the business of owning black slaves. R. Halliburton's *Red over Black* is the first major study of the institution of black slavery among the Cherokee Indians, both in their native Southeast and in the West after Removal. Less than half a percent of Cherokee heads of families were slave-owners. But the more successful of these slave-owners lived in mansions, had their meals served on fine china and silver, sent their sons to Cornwall school or Princeton, called their older slaves "aunt" and "uncle" and their younger slaves "girl" and "boy," and enacted a series of repressive slave codes. In its main features Cherokee slavery was like Southern slavery.

The most important contribution of *Red over Black* is that it lays to rest the notion that Cherokee slavery was less harsh than Southern slavery. It has even been argued that the Cherokees were such lenient masters that their slaves were unfit for use on Southern plantations. Halliburton presents evidence that this myth of Cherokee leniency was encouraged by missionaries, Indian agents, and Indian superintendents, who, for various reasons, wanted to depict Cherokee slavery in favorable terms. His evidence will probably not satisfy everyone. As every historian knows, it is difficult, perhaps impossible, to measure the harshness of slavery. But Halliburton makes it plain that Cherokee masters, like Southern masters, ranged all the way from kind to excessively cruel.

The most serious fault of the book is that it neither describes nor explains the social process which transformed an egalitarian, kinship-based society with a subsistence economy into a stratified and at least quasi-capitalist society. In short, how did the Cherokee slave-owning elite achieve and maintain their privileges?

Small errors mar the book. Halliburton, for example, says that the Cherokee language has no words for "slave" or "Negro." Yet a word-list (c. 1756) in William De Brahn's *Report of the General Survey* contains the Cherokee words *Iwokonéka* "Negro" and *Ulienki* "prisoner, slave." The book could have been more appealingly written and more carefully proofread.

CHARLES HUDSON
University of Georgia

MICHAEL P. MALONE and RICHARD B. ROEDER. *Montana: A History of Two Centuries*. Seattle: University of Washington Press. 1976. Pp. xiii, 352. $14.95.

To historians Montana has a certain archetypal importance among western states. From the age of the mountain men to the Gold Rush, the era of the big beef bonanza, the copper boom, and into mod-

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ern times of mechanized agriculture, strip mining, and the Big Sky project, the Montana country has vividly exemplified the main themes of western history. The region's growth has come in spurts, characteristically dependent upon the exploitation of rich but exhaustible resources. Cycles of prosperity and stagnation have emphasized Montana's continuing reliance upon a few major enterprises, a frontier-like condition that persists today. As elsewhere in the West, seasons of speculative optimism have alternated, irregularly, with periods of shattered hopes; and for those who have stuck it out, this boom and bust pattern has left a deep impress upon all aspects of life. Survival in Montana requires a talent for thinning down in lean times.

Michael Malone and Richard Roeder, members of the history faculty at Montana State University, have set forth the uneven record of Montana's growth with accuracy, honesty, and skill. The authors describe their work as a general, interpretive history, meant for the mature reader and emphasizing "those historical trends and personalities that proved vital to the shaping of present-day Montana." Their scholarship is thorough, their treatment comprehensive. They avoid the merely sensational while displaying an accurate sense of the significant. Although some slangy expressions occasionally mar their style, the book is generally well written.

Montana's economic history receives a detailed treatment. Economic growth, the authors are frank to relate, has come at a snail's pace in comparison with most other states. They offer a strong treatment of Montana politics and the ties between politicians and businessmen, benefiting from Clark Spence's excellent volume on the territorial period. Their account of Indian relations in the nineteenth century (with only brief mention of twentieth-century Indian affairs) reflects the inadequacy of monographic sources, historical and anthropological—an inadequacy which should be seen as a challenge to other scholars.

Altogether, Malone and Roeder have contributed a volume of distinction among western state histories. But one statement challenges credence: the authors boast of Montana's current support for literary and cultural accomplishment. Why then, one might ask, have they not found a Montana publisher? It should be said, however, that the University of Washington Press has given this fine work a first-class job of book production.

KENNETH N. OWENS
California State University, Sacramento

NANCY F. COTT. *The Bonds of Womanhood: "Woman's Sphere" in New England, 1780–1835*. New Haven: Yale University Press. 1977. Pp. xii, 225. $12.50.

This is a thoughtful, scholarly, and well-written study of the early Republic, which touches on broad themes in social and economic history, well beyond the limits of narrowly defined women's history. Through the use of unpublished diaries and letters, as well as through the mentor literature, Nancy Cott shows us how these women saw themselves and their roles. The limitations of the kind of women who left such material and the limitations of the material itself are clearly stated, and, refreshingly, the book makes no claims to cosmic ideology. The quotations are a far more convincing illustration of the book's thesis than pages of rhetoric, and they are intelligently and sympathetically used throughout. The growing secondary material is utilized and assimilated.

The paradox of the "bonds" which simultaneously united and limited women is explored through sections on work, domesticity, education, religion, and sisterhood. The work on education is the least well documented and that on religion the least comprehensive. The exploration of women's contribution to the work force and the analysis of the difference even small amounts of surplus capital made in their lives are particularly valuable. The section on "domesticity" is thorough and a valuable summary of the painful truism that "woman's work is never done." The most original analysis is in the section on "sisterhood" in which the growth and the shedding of these "bonds" are traced.

The exploration of this other "peculiar institution" is a most valuable contribution to the work on the culture and society of American women within the framework of American society which has been done by Carroll Smith-Rosenberg, Gerda Lerner, Mary Ryan, and others. This well-researched monograph is a needed addition to the social history of this critical period.

BARBARA WELTER
Hunter College, City University of New York

SUSAN PHINNEY CONRAD. *Perish the Thought: Intellectual Women in Romantic America, 1830–1860*. New York: Oxford University Press. 1976. Pp. vi, 292. $12.95.

"Strong-minded women" many of the early feminists were called, and indeed only a strong-minded woman could have determined to follow the life of the mind in America in its early years. Yet a group of female intellectuals in the romantic era left its mark on American thought, and Susan Conrad discusses these women.

Conrad proposes the thesis that in responding to romanticism, a philosophy which exalted the self, American cultural life encouraged women to enter into intellectual endeavors and that momentarily, at least, the conflict between the roles of woman

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and intellectual was brought into uneasy resolution. She argues, nevertheless, that the woman intellectual was a distinct type in the period 1830–60, that distinction enhanced by her very necesssity to find her own avenues of expression in the midst of what Barbara Welter has so aptly described as the "cult of true womanhood." This role for women was made more acceptable, Conrad notes, by the women's strategy of conceiving the life of the mind as separate from ordinary life and consequently of not arguing for broader definitions of womanood. In this respect we have much to learn from the experience of these intellectual foremothers, who were allowed intellectual activity but not feminism.

Conrad's method is to examine the lives and works of many of these women. Margaret Fuller is, of course, primary, and Conrad's chapter on her is good. She reminds us how untenable Fuller's definition of the woman intellectual was and yet how prophetic; all later women intellectuals walked in her long shadow. Conrad has been too heavily influenced by Perry Miller's anthology, however, sharing with him the irritating habit of referring to Fuller as "Margaret" and other female intellectuals always by full name. Worse, she has skimped in her research, quoting letters from Fuller to James Nathan from Miller's book when they are available in full elsewhere, and quoting from Miller's abridged *Woman in the Nineteenth Century* and *Summer on the Lakes*. It would be bad enough if this were her only short-cutting, but again and again Conrad uses secondary sources and biographies when primary materials are easily accessible. A letter by Lydia Maria Child is cited from *Notable American Women*, although Child's letters have been published. Her discussion of Beecher's thoughts on women's education is based solely on Barbara Cross' *The Educated Woman in America*, wherein Cross herself notes that she had abridged Beecher's writings.

Rather than rushing her study from dissertation to book form, Conrad's editor should have taken the new field of women's studies seriously enough to correct its flaws: to smooth the prose, find another label for Horace Mann than "educationalist," and avoid describing Fuller as "rather ûnique"; to make sure all citations were consistent and complete with date and page references; to root out the overuse of dashes and of quotation marks, which give Conrad's prose a defensive, self-conscious, breathless look. The book has an interesting and important thesis and introduces the reader to several notable women, but it is so marred by sloppy scholarship, hasty writing, and poor editing that I fear it will go unnoticed.

CYNTHIA D. KINNARD
Indiana University, Bloomington

JOHN S. HALLER, JR. and ROBIN M. HALLER. *The Physician and Sexuality in Victorian America*. Urbana: University of Illinois Press. 1974. Pp. xv, 331. $10.00.

G. J. BARKER-BENFIELD. *The Horrors of the Half-Known Life: Male Attitudes toward Women and Sexuality in Nineteenth-Century America*. New York: Harper and Row. 1976. Pp. xiv, 352. $15.00.

In the nineteenth century in the West, physicians became important as sources of direction for women's behavior in particular and social and sexual conduct more generally. The moral leadership of the medical profession reflected both the clergy's loss of authority and the disintegration of traditional networks of transmission of moral codes. While there is reason to be critical of the use of prescriptive literature as historical source material, and to question what it can tell us about actual practice in an era of rapid social change, it can, if carefully evaluated, reveal much about the search for new ways of living.

Both these books review primarily prescriptive writings, notably those of physicians. While they have some similar weaknesses, their strengths are quite different. John and Robin Haller have compiled physicians' attitudes on a series of topics: the physical inferiority of women, the need for female sexual passivity, proper clothing, proper sexual conduct, sexual diseases, and opium use. The unity of the book stems from the simplicity of its conception: what physicians—an apparently random collection of them—thought, especially what they thought that seems odd to the twentieth-century sensibility. At a deeper level the book remains incoherent, since these attitudes are not organized by the historians into an analysis of a medical worldview, nor an attitudinal set characteristic of any other social group.

G. J. Barker-Benfield's study has less immediately evident unity. It is about Reverend John Todd and physician Augustus Kinsley Gardner, introduced by an interpretation of de Tocqueville's observations on sex roles in the U.S. and by a brief sketch of the rise of gynecology in the U.S. But Barker-Benfield attempts a more amibitous and coherent project: drawing from these four sources an overall interpretation of "male" attitudes towards women and sex in the nineteenth century.

The Hallers have produced a competent collection but little more. They fall, it seems to me, into a trap that awaits many writers about the Victorian "mood" and/or sex history: an essentially ahistorical, entertainment-oriented presentation of oddities. Although acknowledging the diversity and complexity of the Victorian bourgeois mentality, their book attempts no synthesis and is potentially distorting in its neglect of the context of its sources.

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Barker-Benfield, by contrast, has a thesis, although it can be hard to find among the repetitive, overly detailed, and at times inconsistent prose, and although it is an extension and variation of substantial previous work by, for example, Peter Cominos, Carroll Smith-Rosenberg, and Charles Rosenberg. The argument is that sexual conflict, and sex-role differentiation, were greater in the U.S. than in Europe because greater individual competitiveness and mobility here deprived men of traditional sources of ego identity and security (this much is from Tocqueville); that American men sensed an unbearable threat to the social order from their own bursting sexual energy, unchecked by women operating through the family as a stabilizing force; that the resulting male need to contain their own destructive sexual impulses led them to a policy of extreme misogyny and repression of women, as symbols of sexuality, in which policy physician Gardner and minister Todd represented a larger male culture; and that the repression of women simultaneously reflected fear and envy of women's sexual and reproductive powers.

Barker-Benfield's thesis has a compelling logical and psychological sense, but it stimulates some historical doubts. He attributes men's sexual domination of women to men's fear of their own sexuality caused in turn by role insecurity in the New World. Yet this Victorian instability also stimulated women to challenge their subordination, and the historical evidence can also support the argument that it was men's defense against this female challenge, political and personal, that led to increased sexual repressiveness. Barker-Benfield's description underemphasizes the double standard of Victorian sexual attitudes and practices (a hypocrisy and/or dichotomy that was of the essence of Victorian prudery); and equally underemphasizes the changes in women's own behavior. (He accounts for this influence in a mere two paragraphs [p. 123].) Furthermore, in his desire to sympathize with and regret the victimization of women, Barker-Benfield slides into a view of historical change as entirely created by men. He presents women's sexuality exclusively as an interiorization of, or a reaction to, men's wishes. Recent scholarship in women's history reminds us of the dangers of oversimplification in this direction: that even in a system of sexual (or class or racial) inequality, history is "made" by interactions between groups; that from their position in the family women found literally innumerable ways to assert their own interests, even if only in ornery negativeness toward men's desires.

The two books share some methodological problems. Both quote physicians without placing them within the disunified and even conflicted medical profession of the nineteenth century. The Hallers quote "regular" and sectarian physicians interchangeably; Barker-Benfield notes that Gardner was suspended from the New York Academy of Medicine for homeopathy, for example, without explaining the significance of this. Both rely heavily on prescriptive literature without critical evaluation of its significance. Class is a category virtually missing from both expositions. Indeed, neither offers even a careful chronological context, implicitly treating the long Victorian era as if it were unchanging.

Despite these flaws Barker-Benfield's work is a contributon to historical interpretation of an important problem, the study of which could help clarify the troubled relations of the sexes and men's and women's different experiences of sexuality. The Hallers' book provides information that will be useful if the reader can supply the historical context and discrimination.

LINDA GORDON
University of Massachusetts, Boston

JAMES T. MCINTOSH, editor. *The Papers of Jefferson Davis*. Volume 2, *June 1841–July 1846*. Baton Rouge: Louisiana State University Press. 1975. Pp. xxxix, 806. $20.00.

This second volume of *The Papers of Jefferson Davis* covers five years in the life of the future Confederate president. In 1842 he made his first venture into Mississippi politics, in an unsuccessful campaign against Seargent S. Prentiss for the state legislature. Two years later Davis campaigned vigorously for James K. Polk, though he had originally preferred John C. Calhoun as the Democratic presidential nominee. The following year Davis was elected to Congress, where he promptly distinguished himself as a doctrinaire states' rights Democrat, opposed to paper money, protective tariffs, and federally financed internal improvements. Though admirers already spoke of him as "the Calhoun of Mississippi," and "the impersonation of the true spirit of the South," Davis was not yet a sectionalist, and he repeatedly voiced the hope that the Union should "ever be preserved by justice [,] conciliation and brotherhood, without a spot, without a stain of blood that flowed in civil war." Feeling unhappy and overworked as a Congressman, Davis in 1846 resigned in order to command a Mississippi regiment in the war with Mexico.

Personally as well as politically these were important years for Davis. After the death of his first wife, he had retired to his undeveloped farm, "Brierfield," on the Mississippi River below Vicksburg. There, as he wrote, he spent "seven years of almost entire seperation [sic] from the

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external world engaged in clearing fields in the primeval forest of the alluvium of the Mississippi." By the mid-1840s his exertions had begun to pay off. Owning seventy-four slaves and a well-ordered plantation—assessed together at $225,000 in 1860—he had firmly established his fortune. During these same years he met and wooed the vivacious Varina Banks Howell. "Would you believe it," Varina wrote her mother after first meeting Davis, "he is refined and cultivated, and yet he is a Democrat!" In 1845, when Davis was thirty-seven years old and Varina only nineteen, the two were married.

Documentation for this active period in Davis' life is more abundant than for his earlier years, and the editing of these texts exhibits the same strengths and weaknesses demonstrated in the first volume of Davis *Papers*. The coverage is impressive and accurate. Valuable editorial headnotes help clarify Davis' positions on such complex issues as the repudiation of Mississippi state bonds. The notes appended to the documents show a remarkable mastery of Mississippi history and genealogy, and some—such as the account of Brierfield plantation—are fascinating short essays, worthy of separate publication.

On the other hand, this volume, like its predecessor, is cluttered with irrelevant, inconsequential, and unncessary material. Most of the items are not speeches or letters of Jefferson Davis. The editor has chosen to include any and all material that some future biographer of Jefferson Davis might want to consult: letters to Davis, letters about Davis, newspaper announcements of his schedule of political engagements, editorials about his oratorical style, even a Congressional roll call at which Davis was not present. Length of annotation runs in inverse proportion to the importance of the document. For instance, a five-line newspaper announcement of a meeting to organize the Vicksburg Jockey Club is followed by seven closely printed, double-column pages of annotation giving complete biographies of the fifteen other Warren County residents who, along with Davis, signed the notice; for the most part these men had no other connection with Davis and most of their names do not appear elsewhere in his *Papers*.

It would be well if those who prepare the subsequent, eagerly anticipated volumes in this important series would bear in mind the recent injunction of Robert Rutland, the distinguished editor of the Madison papers: "Footnotes rarely endure and may serve an editor's vanity more than a scholar's needs. Thus we would do well to check our impulses, annotate sparingly, and leave the scholar free to make his own interpretations of Clio's wanderings."

DAVID HERBERT DONALD
Harvard University

HARRY P. OWENS, editor. *Perspectives and Irony in American Slavery*. Jackson: University Press of Mississippi. 1976. Pp. xii, 188. Cloth $8.50, paper $3.50.

We continue to be drawn to the subject of slavery in America with a persistence and a tenacity that is startling. After more than two hundred years of analysis and debate, rethinking and revising, it is even more startling that at times new and important things are still being said about the subject.

Pespectives and Irony in American Slavery grew out of a three-day conference sponsored by the Department of History of the University of Mississippi in October 1975, on the subject, "The Slave Experience in America: A Bicentennial Perspective." The volume is composed of papers presented by Carl N. Degler, Eugene D. Genovese, David Brion Davis, Stanley L. Engerman, William K. Scarborough, John W. Blassingame, and Kenneth M. Stampp. The collection illustrates both the difficulty of providing new insights into an old subject and the occasional brilliant breakthrough that is still possible.

The contributions vary enormously, and while not everyone would agree on the merit of individual work, the selections by Davis and Genovese easily dominate the book. In his essay, "Slavery and the American Mind," Davis traces the way that social self-deception and the moral vision of material accumulation combined in the national interest to divert attention from the unpleasant reality of chattel slavery. His extraordinary and original discussion needs to be read entirely to be appreciated. Genovese, writing on "Slavery—the World's Burden," places the American slaveowners in the context of other historically powerful landholding classes in order to analyze the long-range implication of their opposition to the emergence of industrial capitalism. He also sees the resistance of slaves to slavery as preparing the way for the struggle of future laboring classes. This unusual perspective provides a much enlarged landscape upon which to view American slavery and offers a scope that is rare in the writing of United States history.

Carl Degler opens the collection with a survey of "The Irony of American Negro Slavery." Stanley Engerman, in a thoughtful essay on "The Southern Slave Economy," uses price indices as a way of understanding how slaveowners viewed the economic future of their society; he looks again at old questions concerning the system's viability and flexibility and offers fresh insights. In "Slavery—the White Man's Burden," William Scarborough describes slavery through the eyes of the slaveholding class, and in so doing, he reminds us that not all traditional ideas have been replaced in recent years.

John Blassingame, discussing "Status and So-

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cial Structure in the Slave Community," uses slave sources in a fruitful way to assert that social structure and status within the slave community were largely determined by values and traditions within the slave culture and did not reflect the values held by the master class. Kenneth Stampp closes the book with a view of the significance of the American Revolution in the history of slavery in an essay entitled "Slavery—the Historian's Burden." It is a provocative and valuable little book.

ANN J. LANE
Radcliffe Institute

JAMES I. ROBERTSON, JR. and RICHARD M. MCMURRY. *Rank and File: Civil War Essays in Honor of Bell Irvin Wiley*. San Rafael, Calif.: Presidio Press. 1976. Pp. 164. $12.95.

Burgeoning publishing costs have plunged the traditional *festschrift* into hard times. All the more welcome then is this well done, neatly packaged volume of eight essays in well-deserved tribute to Bell Wiley (plus a full bibliography of Wiley's published works prepared by John P. Bloom). In almost a half century of writing and teaching, "uncommon soldier" Wiley, as Henry T. Malone's moving biographical sketch underscores, added a new dimension to Civil War history by illuminating the wartime lives of Southern Negroes (1938), Johnny Reb (1943), Billy Yank (1952), and most recently Confederate women (1975). Further, serving as advisor to those popular history magazines, *Civil War Times* and *American History Illustrated*, Wiley has helped bring history to the general reader.

In addition to Malone, contributors to this volume include Michael B. Dougan on General Thomas C. Hindman, Jr.; Richard M. McMurry on General John B. Hood's early career; Willard E. Wight on Colonel Cyrus B. Harkie; and James I. Robertson on chaplain William Wiatt. All of these essays are ably done, but for this reader, the other three proved especially enlightening. Norman B. Ferris in his "Transatlantic Misunderstanding: William Henry Seward and the Declaration of Paris Negotiation, 1861" clarifies a number of previously obscure aspects of the exchange between Washington on one side and London and Paris on the other. Rejecting the traditional view that Seward was extremely bellicose in dealing with England and France in the spring and summer exchanges of 1861, Ferris shows how conciliatory Seward in fact was in his efforts relating to the proposed American adherence to the Declaration of Paris of 1856. Indeed, he agreed to drop the earlier insistence on the Marcy amendment that would have exempted from seizure the noncontraband private property aboard belligerent ships and was even willing to wink at unofficial efforts by British diplomats in America to get proper treatment for British nationals in the South by Confederate authorities. An incredible series of baseless suspicions, inaccurate reporting, failures to follow official instructions, and outright gaffes by diplomats—British, American, and French—on both sides of the Atlantic led to a total breakdown of negotiations by September.

Moderate Northern Democrats of the Civil War period have received varying degrees of attention from historians. Arnold Shankman here takes a new look at Democratic judge, George Washington Woodward of Pennsylvania's supreme court, who with strong reservations about the war and a son wounded at Gettysburg, ran for governor in 1863 and came within 17,000 votes (out of half a million cast) of ousting Andrew Curtin from the executive mansion. Shankman rightly suggests in his cursory survey that Woodward is "deserving of a scholarly" biography. Maury Klein's examination of 60 randomly chosen Northern industrialists—"boys who stayed behind"—offers an intriguing view of how these men (aged 17 to 30 in 1861) advanced their careers and fortunes during the four years of war.

All in all, this work provides a fitting tribute to a true "gentlemen and scholar" of our profession.

DAVID LINDSEY
California State University, Los Angeles

NORMAN B. FERRIS. *The* Trent *Affair: A Diplomatic Crisis*. Knoxville: University of Tennessee Press. 1977. Pp. xi, 280. $14.95.

The "Trent" Affair supplements Norman B. Ferris' previous book, *Desperate Diplomacy: William H. Seward's Foreign Policy, 1861* (1976), a study of diplomacy during the first year of the Civil War that gives little attention to the *Trent*. This new book is detailed narrative history, describing everything from the furniture of each room in which Mason and Slidell were confined at Fort Warren to a Turin appraisal of the settlement. Quotations, often long, abound. Seward is the hero, as in the earlier volume. The principal British leaders, on the other hand, are treated with little favor.

There is nothing wrong with purely narrative history, and many sections of Ferris' book give interesting accounts of exciting events. But in the preface he expresses an intention to do more than narrate: he wants to fill the gap left by historians who have not explained "*why* a relatively trivial maritime incident threatened to touch off a trans-Atlantic war at the end of the year 1861." (How-

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ever, in *Desperate Diplomacy,* Ferris claims that he, at least, has already filled the gap by answering the question, "Why did this essentially trivial misunderstanding impel the people of two great nations to ready their weapons of war?") Ferris does point to misleading reports about Seward that persuaded Palmerston and Russell of the secretary's warlike intentions, but he does little to analyze the British point of view. If Henry Adams could write: "What Seward means is more than I can guess. But if he means war . . . Mr. Seward is the greatest criminal we've had yet," the British leaders cannot be censured for similar feelings. Anyway, was the secretary quite as blameless as portrayed? If the British had known about his April 1 suggestions to Lincoln (a matter neglected in this book and virtually neglected in *Desperate Diplomacy*), they would have been even more concerned than they were. Moreover, the many basic differences between the two trans-Atlantic societies, and the years of Anglo-American friction culminating in two wars, were bound to add to London's alarm over Captain Wilkes' provocative action.

In short, *The "Trent" Affair* is less successful as analysis than as narrative. One would like to know, for example, how great the possibility of war was "over what was essentially a minor incident." Already by December 16, when emotions were still high in Britain, Russell did "not think the country would approve an immediate declaration of war," and without popular support the government would have hesitated to take the plunge. Perhaps the war danger was really no greater than it was over Venezuela in the mid-1890s, when a sudden flare-up soon subsided; and in 1861, as in 1896, British religious, arbitration, and peace societies sprang into action to oppose hostilites. In any case, what were the motives of even the most anti-American members of the British Cabinet? Did they want to seize the opportunity to ensure the breakup of the United States? Were they merely swept along by the initial clamor? And what about the rights and wrongs of the legal issues? What is Ferris' opinion about the legality of Wilkes' action, and what does he think of Seward's legal arguments and Russell's reply? Consideration of these and other such questions would enhance the book's value.

All this does not detract from the interest of much of the narrative. The book is well researched. The footnotes, unfortunately placed at the end, are thorough (curiously enough, they frequently give more analysis than the text). And there cannot be much about the *Trent* that is omitted from the twenty-two-page critical bibliography.

CHARLES S. CAMPBELL
Claremont Graduate School

HERMAN BELZ. *A New Birth of Freedom: The Republican Party and Freedmen's Rights, 1861 to 1866.* (Contributions in American History, number 52.) Westport, Conn.: Greenwood Press. 1977. Pp. xv, 199. $16.95.

The purpose of this compact, nicely researched study is to describe "how and why the Republican party transformed a series of expedient military steps aimed at denying slave manpower to the enemy into a civil rights policy that rested on settled constitutional principles and was intended to guarantee American citizenship and equality before the law to the freed slave population." Herman Belz hews, however, so closely to a narrow type of constitutional and legislative history that he minimizes larger forces that operated on Republicans in Congress, and the result is a sparse narrative that tends in places to be tedious. Furthermore, at the very outset the reader encounters the hoary half- (or quarter-?) truth that Lincoln's restraint concerning slavery during the first year of the war was rooted in his fear of losing the border states to the Confederacy. The implication of this familiar argument is that "the North" panted from the outset to destroy slavery. Belz well understands that the matter was not so simple, for early on he has to deal with the foreign colonization of blacks as proposed by Lincoln and embodied in the second Confiscation Act. For this development the border states lose their great utility as controlling factor, and one discovers that colonization "would allay apprehensions about large-scale migration of blacks into the North" (p. 13). Indeed, Belz subsequently argues that because of those Northern fears "the first and most fundamental antislavery answer to the question of post-emancipation policy was to insist that the freedmen be left alone to remain where they were—on the soil of the southern states" (p. 70).

The progression from civil rights for the freedman in foreign tropics, as called for in the second Confiscation Act, to the Fourteenth Amendment is a fascinating one, even if the steps of the Republicans were halting and their motivation vastly mixed. After delineating the various wartime steps and the final conflict between Lincoln and the Republican supporters of the Wade-Davis plan for reconstruction, Belz well summarizes three fundamental considerations that came together in the settlement of the civil rights issue after Appomattox: 1.) the basic Republican conception of civil rights, which focused on legal equality as distinguished from political and social equality; 2.) motivation that continued to be, as it was during the war, "a blend of humanitarian idealism and pragmatic political and economic concerns, tempered by sensitivity to northern racial prejudice"; and 3.)

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a theory of federalism that became more important after the war had ended. On this last point Belz concludes that the preponderance of evidence suggests that the framers of the Fourteenth Amendment meant for "the states to remain the primary protectors of civil rights against private wrongs" and for the federal government to "intervene legislatively only when states denied rights by passing discriminatory laws or by failing to protect citizens" (p. 173).

ROBERT F. DURDEN
Duke University

THOMAS L. CONNELLY. *The Marble Man: Robert E. Lee and His Image in American Society*. New York: Alfred A. Knopf. 1977. Pp. xv, 249. $10.00.

Most Americans may know vaguely that Vicksburg and Chattanooga figured in the Civil War, but they are likely to envision the conflict as something that happened in Virginia and ended at Appomattox when Robert E. Lee surrendered. "In every era of Civil War writing," Thomas Connelly asserts, "a cult of Virginia authors has dominated Confederate letters and provided the Lee image with a base of publicity enjoyed by no other Southern hero" (p. xiii).

Civil War buffs will know that not all Confederates took kindly to the fine airs of certain Virginians. And if Connelly's study in mythmaking sounds echoes of a distant feud, his background as a Tennessean who, after writing two volumes on the Army of Tennessee, knows that decisive actions took place elsewhere gives him a useful vantage point from which to penetrate the legend of Lee.

The image of the spotless and invincible Lee served many causes. The renamed Washington and Lee College used it in fund-raising. The vanquished found balm for defeat. Some found escape from personal obscurity or wartime failures. Writers made it into a profitable industry.

During the 1870s Virginians led by Jubal A. Early, whose devotion was "an alchemy of gratitude and guilt" (p. 52), made the Southern Historical Society over into an agency of the Lee mystique. The image they created is familiar, for it affected all later treatments: the aristocrat sprung from cavalier Virginia, son of a Revolutionary hero, devoted to his mother, joined in idyllic marriage to Mary Custis, step-granddaughter of Washington, a model soldier obedient to duty and self-denial, a man of sorrows who loved Virginia but hated slavery and secession, the supreme military genius overrun by numbers, at the end a Christ-like figure of sacrifice and endurance.

Eventually the regional hero became the hero of a national epic, an example of character to a nation troubled by the loss of traditional values, and reached his ultimate apotheosis in Douglas Southall Freeman's monumental biography. Like other devotees Freeman resisted all hints that Lee's legendary reserve marked "some deep spiritual conflict."

Connelly enters a dissent. In Connelly's psychological profile Lee is no longer Stephen Vincent Benét's "marble man," safe from the "picklocks" of biographers. He becomes instead a man born to trouble, into a family which sank into debt, scandal, and disgrace. Much of his pinched childhood was spent caring for a sickly mother. And Mary Custis, "the spoiled child of Arlington," became a neurasthenic, complaining wife. On remote tours of duty, beset by homesickness and a sense of failure, Lee badgered his children with letters that instilled a sense of inadequacy and guilt.

After the Mexican War he seems to have adopted as a defense against self-doubt the rigid code of behavior that his champions celebrated. "Duty became obsessive," along with self-denial, religion, and a fixation on death and otherworldliness. His often costly audacity in combat, Connelly suggests, may have been the product of a repressed personality, an emotional release for inner turmoil.

There is much more than one can report in limited space. Yet, while Connelly debunks the myth, he does not debunk the man. "Heroes are not made of nothing, and Robert Lee was a man of more substance than many others . . . a superb general . . . a man of lofty character whose demeanor transcended the pettiness of many other men. Lee did not require a cult of admirers to establish his reputation." All in all, by restoring his humanity, Connelly has served it better than they.

GEORGE B. TINDALL
University of North Carolina, Chapel Hill

JOHN Y. SIMON, editor. *The Papers of Ulysses S. Grant*. Volume 6, *September 1–December 8, 1862*. Carbondale: Southern Illinois University Press. 1977. Pp. xxiv, 492. $17.50.

This sixth volume of the correspondence of Ulysses S. Grant covers a seemingly dull period in the general's Civil War career. Lying between the Shiloh and Vicksburg campaigns, the weeks of September to December 1862, might appear to have been comparatively uneventful. Such was not the case at all. The autumn months of the war's second year saw Grant launch his first major advance on Vicksburg. Successes at Iuka and Corinth increased Union optimism, but Confederate coun-

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terattacks—notably at Holly Springs in December—brought Grant's thrust at the Mississippi River fortress to a momentary but frustrating halt.

Meanwhile, stabilizing the high command of one of the largest armies America ever assembled posed a number of personnel problems for Grant. During this time he developed increased confidence in such generals as the youthful James B. McPherson and temperamental William T. Sherman. Yet Grant's departmental commander, Henry W. Halleck, seemed to vacillate to the point of inspiring mistrust. In addition, Grant considered Gen. John A. McClernand to be "unmanageable and incompetent"; while another subaltern, Gen. William S. Rosecrans, was both disappointing and conniving. Such officers became increasingly difficult to tolerate, but Grant had not yet determined how to get rid of them.

Grant's letters in the fall of 1862 reveal his concerns over mobilization, Confederate buildups, jealous associates, and ideas for a new invasion of Mississippi. This was obviously a busy time for the plain-looking field commander. Before Christmas he issued a directive against Jewish merchants that would have repercussions for over a century; and in the spring months that followed, Grant embarked upon his most critical campaign in the western theater.

Volume 6 of the general's letters reflects the scholarly excellence found in earlier installments. Editor John Simon has insured that each letter is fully documented and annotated. A map, a chronology of events for this time segment, five portraits, a descriptive calendar of minor correspondence, and a name-and-place index provide as many extras as any researcher could want. This series, an outgrowth of the Civil War Centennial, remains one of the most authoritative legacies of that commemoration. How regrettable it is that such qualitative compilations exist for so few notables of that embattled age.

JAMES I. ROBERTSON, JR.
Virginia Polytechnic Institute and State University

JOSEPH H. PARKS. *Joseph E. Brown of Georgia.* (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1977. Pp. x, 612 $35.00.

Joseph E. Brown, described by the author of this biography as "the most influential man in Georgia" from the late 1850s until the end of the 1880s, clearly played a pivotal role in his state's political life. Born in 1821, Brown rose from yeoman origins in the north Georgia hills to the governorship at the end of the antebellum era. As the state's chief executive from 1858 to 1865, he immediately became a storm-center of controversy, espousing a Jacksonian hostility toward banks, campaigning for the creation of public schools, and pressing relentlessly for Georgia's secession from the Union. With the outbreak of the Civil War, Brown's combative personality and commitment to states' rights led him to clash with the Confederate central government, and he soon became a leading critic of Jefferson Davis' handling of the conflict. Shelving his old secessionist loyalties after Appomattox, he urged war-ravaged Southerners to reconcile themselves to Radical Republican demands for black political equality. Brown subsequently joined the Republican Party—a move which provoked howls of condemnation from the state's traditional leadership. Recognizing that the Radical hegemony in Georgia would be short-lived, however, the political pragmatist shifted positions once again and returned to the Democratic fold in the early 1870s. Old antagonisms were soon muted, and Brown, now a Democratic loyalist, won a seat in the United States Senate in 1880 and held it throughout the decade.

Joseph H. Parks' lucid account of these developments is authoritative in character and based upon in-depth research in manuscript and newspaper sources. His writing style is direct and forceful, admirably clarifying the intricate patronage feuds, personality conflicts, and backstairs intrigues which dominated mid-nineteenth-century Georgia politics. Of particular interest are the book's perceptive accounts of Brown's efforts to protect and provision Georgia's people during the Civil War, his abortive peace negotiations with General William Tecumseh Sherman in 1864, and his ethically dubious collaboration with federal occupation authorities immediately after the conflict. Although justifiably critical of Brown's opportunism and personal vagaries, Parks maintains a balanced, dispassionate tone throughout his analysis.

Despite these virtues, this biography suffers from several significant defects. Perhaps most important, the book is markedly uneven in its treatment of the various phases of Brown's life, offering relatively few insights into Brown's youth, senatorial career, or retirement years. Indeed, Parks skims over the first twenty-eight years of the politician's life in less than six pages, thus effectively ignoring a period which may well hold the key to Brown's complex, contradictory character. The author further subjects himself to criticism by padding his lengthy account with an excessive number of quotations and concerns himself too much with minutiae. Such shortcomings prevent a good biography from being an excellent one.

JAMES T. MOORE
Virginia Commonwealth University

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RONALD L. NUMBERS. *Creation by Natural Law: Laplace's Nebular Hypothesis in American Thought.* Seattle: University of Washington Press. 1977. Pp. xi, 184. $15.00.

Ronald L. Numbers' objective is to demonstrate that the nebular hypothesis of Pierre Simon Laplace was an important step in the development of a receptive climate for naturalistic explanations of the origin of physical and biological phenomena. Numbers claims that his work shows that the nebular hypothesis "came to enjoy widespread popularity with educated Americans, including the nation's most prominent scientists, well before the appearance of Darwin's *Origin of the Species* in 1859" (p. vii). Ultimately then, Laplace's hypothesis, along with other naturalistic theories, helped to pave the way for the rapid acceptance of organic evolution in the last part of the nineteenth century.

Numbers develops his thesis first by providing a brief survey of the naturalistic explanations for the formation of our solar system from the time of Newton to that of Laplace. He then indicates some of the scientific questions raised and evidence offered, especially by American scientists, to support the nebular hypothesis. Finally, he notes the scientific movement away from Laplace's hypothesis in the 1870s.

This discussion of the place of the nebular hypothesis in the overall development of naturalistic explanations and the various reactions it produced constitutes the major part of the book and serves as the foundation upon which Numbers now proceeds to build his main argument. It was the acceptance of the nebular hypothesis as a strong scientific possibility which forced American scientists, as well as other religious persons, to find a way to incorporate such naturalistic explanations of the world's creation within the context of God's providence. Thus, when Darwin's *Origin* appeared, many of the intellectual adjustments necessary for its acceptance had already been proposed by the supporters of the nebular hypothesis.

However, this book contains several serious weaknesses. Numbers mainly restricts his attention to noting the kinds of statements which scientists made for publication rather than explaining the intellectual process which led to these final statements. He tends, for example, to ignore the larger philosophical significance of Laplace's *Celestial Mechanics* as the zenith of mathematical physics based on Newton's planetary theories. Thus, during much of the nineteenth century Laplace's nebular hypothesis represented a further extension of the mechanistic explanations of nature as well as contributing later to the acceptance of the idea of organic evolution.

Another example of the superficiality which results from Numbers' avoidance of consideration of personal values is his treatment of Simon Newcomb, America's foremost positional astronomer of the nineteenth century. Numbers neglects the significance of Newcomb's excitement and inspiration as a young scientist in being able to understand and further his explanation of nature. Had Numbers gone more deeply into these motivations, he could have conveyed a greater understanding of Newcomb's belief in the utilization of mechanistic explanations for social as well as scientific questions.

Even more difficult to understand in a book about the influence of the nebular hypothesis on American thought is the fact that Numbers totally ignores the strong effects of the determinism implied by such naturalistic ideas on such thinkers as William James. James as a young man had suffered a severe depression when faced with a world so completely determined. In rebellion, he developed the ideas of free will that are in his pragmatic philosophy. It is unfortunate that when Numbers uses the term "American thinkers" he tends to refer only to physical and natural scientists, and that when he refers to other educated persons he usually means religious persons.

Many times one hears the truism of the influence of science on thought and society. The reader would be severely disappointed, however, if he or she relied on this book to understand the impact of an important example of naturalistic-mechanistic thinking in the nineteenth century and its relationship to the acceptance of organic evolutionary ideas.

HERBERT WINNIK
St. Mary's College

CYNTHIA EAGLE RUSSETT. *Darwin in America: The Intellectual Response, 1865–1912.* San Francisco: W. H. Freeman. 1976. Pp. ix, 228. Cloth $9.00, paper $4.95.

This volume, to quote the author's intentions, is "in the nature of an essay. . . to provide for undergraduates an intelligible account of the impact of Darwinism on American intellectual life." Cynthia Eagle Russett also affirms she has avoided research "in manuscript or obscure sources" in order "to focus on the published record as the arena in which controversial issues were thrashed out." The analyses that flow from this purpose and method raise important questions for pedagogy and the study of intellectual history. By what logic does the published record alone reveal the syncretism between individual creativity and the public life of the mind, especially when, as with the American debates over evolution, published sec-

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ondary and original sources available give significantly different interpretations to the events reported?

These caveats aside, Russett's contribution must be classed as minor. Except for excellent analyses of the influence of Darwin on the thought of Veblen, Dewey, and Dreiser, there is nothing in this slim book that is not readily available in general intellectual histories. In the area of the Darwinian impact on America, a professor would be better advised to direct students to the studies of Bert Loewenberg, Richard Hofstadter, Henry Steele Commager, Alfred Kazin, Eric Goldman, Morton White, Paul Boller, or Phillip P. Weiner, all acknowledged in the author's notes and bibliography. What is presented here, then, is a diligently and dutifully written synthesis of accepted verities about Americans who were influenced by Darwin. Agassiz, Gray, Fiske, James, Dewey, Wright, C. S. Peirce, Henry Adams, Veblen, Frank Norris, Theodore Dreiser, and Jack London, are thus all duly catalogued.

Russett's theory of intellectual history seems to be that there must be a correlation between an "idea" (Darwinism) and its impact on an individual, and this correlation is discoverable in the record. But that record as reported in this volume is selective; it also treats the response to evolution almost as a universal aspect of American life and thought and assumes that individuals such as Royce or Santayana who were not part of this mainstream are assignable to some unspecified subdominant cultural pattern.

A first-rate aspect of the volume is the author's analyses of determinism in American ideas. This extends beyond the biological mode, and could have provided the focus for a more catholic work on concepts of force, energy, and determinism, subjects that comprise a holistic philosophy of science and culture in the period the author treats.

EDWARD LURIE
University of Delaware

ROBERT C. POST. *Physics, Patents, and Politics: A Biography of Charles Grafton Page*. New York: Science History Publications. 1976. Pp. 227. $15.95.

This slim biography presents the life story of a man whose private manuscripts no longer exist and whose early accomplishments have been largely obscured by the vicissitudes of his later years—and more. Biographer Robert Post pursues with skill and ingenuity the question of why Charles Grafton Page suffered from contemporary decline and scholarly negligence. He finds some answers in the nature of the mid-nineteenth-century scientific community and others in our discipline's approach to historical analysis. The resulting book is provocative and interesting, if not fully satisfying.

Page was an exceptionally intelligent and energetic young man. Like his contemporary Joseph Henry, he experimented with electricity and electromagnetism and gained a substantial reputation for his work on coils in the 1830s and 1840s. About the time Henry became head of the Smithsonian Institution in Washington, Page joined the United States Patent Office as examiner. Then the parallel paths diverged. Henry devoted his attention to the promotion of scientific investigation and established himself as leader in efforts to professionalize science. Page, on the other hand, turned his genuine curiosity about science and technology toward profit-making ventures, becoming involved with electric locomotives and eventually leaving the Patent Office to become a patent agent for other incipient entrepreneurs. Financial problems, impolitic decisions, and finally ill health undermined Page's grand hopes for lasting fame as well as his contemporary status.

Historians of technology may be disappointed with the relatively little attention given to the inventive, engineering community. Post relies on the *Scientific American* as a source of technologists' attitudes toward Page and Patent Office policies, but little is said about the position of technological schools or emerging professional societies. Historians of science, however, will be fascinated by the display of infighting and antiscience rhetoric that was building in the 1850s. Post traces the slippage of Page from the scientific community and in the process reveals the complex world of experimental science. Although Page had come of age during a period when theory-building, problem-solving, and technological tinkering seemed a part of a "community of interests," that consensus was eroded by the demands both of inventors demanding more liberal interpretations of originality in patent claims and of scientists seeking to establish stricter standards for "original" research publications.

Familiar with each side and fully committed to neither one, Page fell into the growing chasm between them. Post is sympathetic to Page's dilemma and effectively rehabilitates Page's reputation through his careful assessment of the electrical research. At the same time he provides a fresh, less Whiggish look at the process of professionalization in science. Editorial tone, stylistic problems, and psychological speculations are occasionally distracting. Still, there is a vigor to this inquiry which makes it an unusually readable biography and an important addition to the study of midcentury American science.

SALLY GREGORY KOHLSTEDT
Syracuse University

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REESE V. JENKINS. *Images and Enterprise: Technology and the American Photographic Industry, 1839 to 1925.* (Johns Hopkins Studies in the History of Technology.) Baltimore: Johns Hopkins University Press. 1975. Pp. xviii, 371. $20.00.

From the introduction of the daguerreotype into the United States in 1839 through the latest wrinkle in instant movies, Americans have been in love with photography. At least part of this affection probably stems from the fact that photography is so obviously and precisely technological—optics and chemistry embodied in mechanisms and processes both mysterious and comfortingly specific. In *Images and Enterprise* Reese V. Jenkins has ably and probably definitively captured the technical and business history of this important industry.

The book is, at the same time, business, economic, and technological history. It is Jenkins' belief that the "distinctive product technologies" rather than market changes moved the industry from one stage of development to another, and his division of the book nicely makes the point: Daguerreotype Period, 1839–55, Collodion Period, 1855–80, Gelatin Plate Period, 1880–95, Period of the Amateur Roll Film System, 1895–1909, and Period of Silent Cinematography, 1909–25. Each of these is defined by the product technology, and each saw different firms, market structures, and economic activities. It will come as no surprise that George Eastman dominates the story from the later nineteenth century onward, and it is one major triumph of the book to have used and mastered the manuscripts of the Eastman Kodak Co.

In the 1890s Eastman was a pioneer among American businessmen in wishing to identify his company's name with science, and Jenkins' description of the entrepreneur's role in institutionalizing technical change (as in the establishment of industrial research laboratories) adds significantly to our meager knowledge of this very modern process.

Finally, one must acknowledge the fine work of the Johns Hopkins Press in producing a book at once so informative and attractive. *Images and Enterprise* is lavishly and cleverly illustrated with the pictures needed to understand the technology and enjoy the flavor of the enterprise.

CARROLL PURSELL
University of California, Santa Barbara

ROBERT BRUCE DAVIES. *Peacefully Working to Conquer the World: Singer Sewing Machines in Foreign Markets, 1854–1920.* (American Business Abroad.) New York: Arno Press. 1976. Pp. 390. $30.00.

This volume is part of the Arno Press collection on American Business Abroad: Origins and Development of the Multinational Corporation. The series is made up, for the most part, of previously unpublished doctoral dissertations and reissues of out-of-print works. Robert Davies' book is his 1967 University of Wisconsin thesis, with the addition of a short preface. The work is based on company files now deposited at the State Historical Society of Wisconsin. These records are far from complete, since information about finances and some important foreign operations are not included in the collection. The sources available dictated that the history be written from the perspective of the firm's senior officials.

In the first half of his study, Davies presents an overview of company policy between 1851 and 1920. He attributes the firm's success to a patent pool, innovative marketing, continued attention to technological improvements, and the development of specialized lines of machines. The second half of the book is devoted to case studies of foreign operations in India, China, Turkey, Russia, and Germany. He illustrates how the company adapted its marketing skills to very different areas and how it dealt with nationalistic European economic policies.

Davies brings his work of a decade ago up to date in his preface by pointing out its wider implications. He draws attention to the fact that Singer did not seek world markets as "a safety valve to handle an occasional domestic surplus" (pp. vi–vii). Moreover, by 1900 Singer had a foreign organization that for the most part made governmental assistance unnecessary. Indeed, in markets where local competitors campaigned against foreign manufacturers, Singer avoided contact with American officials, hoping to mask the company's nationality.

Informative as this work is, the volume suffers from Davies' decision to study first the administrative history of the company and then its foreign operations. The two are so closely related that the work becomes repetitious. Nevertheless, this book is an important contribution to the history of the multinational firm. And it should find an audience beyond business and economic historians because Davies provides useful insights to students of American expansion and foreign policy.

WILLIAM H. BECKER
University of Maryland, Baltimore County

JOHN F. STOVER. *History of the Illinois Central Railroad.* (Railroads of America.) New York: Macmillan Publishing Company. 1975. Pp. xiv, 575. $15.00.

The Illinois Central Railroad in 1850 obtained the first railroad land grant made by the United

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States. This "main line of mid-America" by 1971 had grown by construction and merger into a railroad system with 9,491 miles of track, centered in Illinois and Mississippi but also reaching Birmingham, Mobile, Montgomery, New Orleans, St. Louis, Kansas City, Sioux Falls, Madison, Indianapolis, Evansville, Louisville, Nashville, Memphis, and Shreveport. Stephen A. Douglas, Abraham Lincoln, and other prominent Americans were associated with its beginnings. The company was blessed from the start with a favorable location and able management. It has never suffered from the machinations of financial buccaneers, and it has never been in receivership, although the depressions of the 1870s, 1890s, and particularly the 1930s subjected it to severe stresses. From the early days until 1931 the company never missed a dividend; then for nineteen years it did not pay one.

At last we have a study of this important enterprise by a professional historian with credentials established in the railroad field. The continuity of ownership and control, uncomplicated by piratical manipulations, makes the company's history attractive to study as an illustration of the phases of railroad experience in the United States. The author covers a broad sweep of time and presents a vast amount of information. He emphasizes description rather than interpretation or analysis. Of special interest to this reviewer is the elaborate examination of the company in the years of depression, war, and inflation, from 1929 to 1950, for the company's experience throws light upon that of the country's other railroad companies in these same troubled and turbulent years. The company in the 1960s fathered a holding company, Illinois Central Industries, which now owns the parent and other extensive but unrelated enterprises as well. That is to say, the company has metamorphosed itself into a conglomerate.

The development of a railroad company is so enmeshed with the surrounding social, political, and economic forces and with persons and institutions that the study of railroad history can provide a useful approach to an understanding of the controlling forces in American history. There is difficulty, however, in finding a main theme to support a narrative approach. John Stover's solution to the problem is in the use of a loose chronological approach as his "main line," and his book becomes something of a chronological compendium. When he has a good story or some interesting side information, he just throws it in, then later returns to his main narrative. Repetition and trivial digressions, e.g., proceedings and menus at company banquets, clutter the book. Nevertheless, Stover's basic system works, and the book is quite readable. Its limitations arise primarily from the difficulties inherent in dealing with a story of such magnitude.

JAMES F. DOSTER
University of Alabama

CARL W. CONDIT. *The Railroad and the City: A Technological and Urbanistic History of Cincinnati*. Columbus: Ohio State University Press. 1977. Pp. xii, 335. $15.00.

The Railroad and the City is a remarkable book by an exemplary scholar and should be read by all those interested in the development of American urban civilization. This monograph, like all significant works of scholarship, operates simultaneously on several different levels. Its most obvious contribution is the careful documentation supporting the thesis that "transportation technology at the mid-nineteenth century shaped urban growth." In five data-packed and compelling chapters Condit has pieced together, from primary sources, the story of how Cincinnati's natural environment was altered by a succession of often overlapping changes in basic transportation systems and the technologies which made them possible.

With careful detail Condit discusses the impact of canal building, of the steam railway, of the construction of links with the South, of the coming of local lines and the street railways, and of electrification. Of particular interest is his description of the rapid rise and decline of the energy-efficient and socially effective inter-urban system of transportation, which by 1919 carried about nineteen million passengers annually in Cincinnati alone. Although Condit is correctly critical of the role of the automobile and bus in the death of this system, he does not mention—perhaps because the governmental evidence surfaced after his research had been completed—the process by which Detroit penetrated such electrified systems for the purpose of destroying them and then sold American cities less efficient and socially destructive high-energy-consuming machines. By the end of the book one can only be convinced of the thesis, already convincingly argued by Sam Bass Warner and others, that much of the shape, tempo, and feeling of American cities was—and is—conditioned by its systems of transportation.

This point is made all the more compelling by Condit's complete understanding of railroad technology and of building architecture and by his ability to relate such technology to the evolving shape of the city. This is reinforced by interspersing visual materials in such a way as to constantly illuminate his major points. More importantly, Condit, a humanist, is constantly concerned with the urbanistic aspect of transpor-

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tation systems and their relationship to the topography and land-use patterns of the city as well as, to the visual and psychological elements of urban structures; that is to say, how structures such as train stations are perceived by the average citizen. Condit's discussion of the architecture of the Cincinnati Union Terminal, a rejection of classical order for a more picturesque and colorful Art Deco style, is a particularly important chapter in architectural history.

This book also operates on an equally significant level of transportation planning policy for today. It demonstrates, as only studies of individual cities can, the usefulness of historic analysis in addressing the most conspicuous failure of American civilization—our rotting urban centers. There can be no doubt, based on Condit's work—and on what we already know—that the failure to adopt and implement a rational, balanced national transportation system was one major factor in the decline of American cities. This awesome fact is made clearer by Condit's point that when during periods of national emergency—World War I and II—the national government was called upon to perform in the field of railroad production and management it did amazingly well. Condit concludes that "the need for government control demonstrated the great fundamental cause of all the problems, namely, the absence of a national railroad system and a national transportation policy" (p. 177).

Most instructive and indicative of the usefulness of knowing the past is Condit's belief that Cincinnati's transportation past contains examples of imaginative, socially constructive, and physically sound planning which were never implemented. These projects remain, like similar examples which must exist in every American city, as case studies applicable to today's problems. Condit ends his book with these words about the future of transportation planning in relation to the city of Cincinnati: "It is a curious irony that a station plan of 1910 and a rapid transit plan of 1926 offer once again the starting point for productive thinking about the rebuilding of the modern industrial city for human use."

ALBERT FEIN
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MICHAEL F. FUNCHION. *Chicago's Irish Nationalists, 1881–1890.* (The Irish American.) New York: Arno Press. 1976. Pp. 160. $11.00.

In this political essay on Irish-American nationalism Michael Funchion illuminates the role of the highly secretive Clan-na-Gael or United Brotherhood during the 1880s and emphasizes the prominence of Chicago's Irish among Clan leadership. But Chicago is actually more incidental to much of this study than Funchion would have us believe. Three or four Chicagoans played active leadership roles in the Irish-American nationalist movement of the 1880s, and the conflict within that movement had its local political front. But the strength of the Clan's political machine in Chicago is argued rather than demonstrated effectively (indeed, that would be a tremendous undertaking in comparative urban history). In any case, the book's strength lies elsewhere.

The last half of this compact volume details the Clan's intervention in and dominance over nationalist activities during the 1880s. By 1883 the American Land League had been dissolved and reorganized as the Irish National League of America, "secretly under the control of the Clan-na-Gael" (p. 79). Funchion then documents the conflict within Clan ranks: on one side, the "Triangle" (a three-man executive council led by Alexander Sullivan, a power broker in Chicago politics), supporting an "extremist" dynamite campaign in England, controlled and manipulated Clan meetings and funds: against them, the Devoyite anti-Triangle forces were committed to constitutional movements like Home Rule. Funchion traces this conflict and each group's response to Irish and British politics through the decade, culminating in an analysis of the secret Clan trial of the Triangle group in 1888. The book concludes with a detailed investigation of the murder of Doctor Patrick Cronin, the anti-Triangle leader from Chicago.

As political narrative, Funchion's book is informative; unfortunately, the first half of the volume tries to place this material in a larger social context, and this effort is much less satisfactory. Funchion relies too heavily on secondary material and reduces his argument to poorly substantiated psychologizing. For example, he attributes Irish-American nationalism to a "massive inferiority complex" (p. 15). Thus, Irish traditions of resistance to English political colonialism and a history of social exploitation are reduced to Irish psychological problems. As such, nationalism is not protest, it is maladjustment.

Nonetheless, for the political narrative alone, this book is a welcome historical addition. The volume's usefulness would be greater still if the author had compiled an index.

DANIEL J. WALKOWITZ
Rutgers University,
New Brunswick

RICHARD STIVERS. *A Hair of the Dog: Irish Drinking and American Stereotype.* University Park: Pennsylvania State University Press. 1976. Pp. 197. $13.50.

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Richard Stivers asks a number of interesting questions about the ethnic identity of Irish-Americans. Why do the Irish have such a reputation for prodigious drinking? Can sociological labeling theory be used to explain the persistence of this stereotype throughout the nineteenth and twentieth centuries in the United States? What do statistical indices of drunkenness and alcoholism reveal about the Irish propensity to drink? How did the immigration experience affect Irish drinking habits?

In trying to answer these questions, Stivers borrows heavily from an eclectic group of scholars, ranging from Claude Levi-Strauss to Edwin Lemert to Philip Rieff. He then applies their theoretical constructs to the data that have been collected by a number of historians, such as Brian Harrison, Robert Ernst, and Oscar Handlin. Unfortunately, Stivers' attempt to weld together the insights of several disciplines results in a disjointed product, which offers few new perspectives on either the process of cultural stereotyping or the temperance/alcoholism conflict. Historians will be disappointed by Stivers' tendency to rely heavily upon already published research related to the lives of the nineteenth-century Irish. Sociologists might well criticize his tendency to vacillate between treatment of Irish drinking as an aspect of a societal label and attempts to measure the actual drinking habits of the nineteenth-century Irish by using highly unreliable medical and crime statistics. He also borrows heavily from the fictional works of novelists such as Stephen Crane to support broad generalizations about Irish-American life.

Perhaps most interesting is Stivers' comparison of the cultural context and function of drinking in Ireland with that of immigrants in the United States. He notes that in Ireland drinking was an important part of what he calls an "avunculate," a society dominated by bachelor males (p. 84). Drinking served as a form of cultural remission; it provided males with a release from a sexually constrained society. Hard drinking, Stivers claims, was integrated into almost every aspect of the Irish work world (a situation that Brian Harrison's book on drink in Victorian England suggests was not unique to the Irish).

In contrast, drinking in the United States served as a form of social control. It kept workers bound to employers and political machines. Instead of helping them to escape society, it bound them more closely to it by reinforcing American stereotypes of the constantly drunk shanty Irish.

Perhaps because he relies so heavily upon writings of temperance leaders, Stivers' contrast of Ireland with the United States lacks substance. Nonetheless, his model for treating what he calls a "dialectical interplay of cultures and cultural definitions" remains interesting (p. 164). He asserts that, by equating masculinity with hard drinking, Irish culture created a group of immigrants highly susceptible to their stereotype as alcoholics in American society. Paradoxically, by the 1880s, this stereotype had begun to further the assimilation of the Irish-Americans into American culture. As nativist groups reacted against East European immigrants, they began to feel more positively about the earlier immigrant groups. The cartoon portrayal of the Irish shifted from simian-like degenerates to likeable, happy drunks. With the evolution of this less pernicious stereotype, the Irish began to be accepted into American society, albeit into a rather narrow niche.

ELLEN DWYER
Indiana University, Bloomington

ROBERT HIGGS. *Competition and Coercion: Blacks in the American Economy, 1865–1914*. (Hoover Institution-Publication, number 163.) New York: CambridgeUniversity Press. 1977. Pp. x, 208. $14.95.

This slender volume (only 134 pages of text) surveys and explains the economic history of American blacks from emancipation to 1915 in prose so nimble and quick that no reader can miss Robert Higgs' two messages: that blacks' first fifty years of freedom were years of diversity and economic improvement, rather than of unrelieved and universal peonage, and that at least one new economic historian can write well. Such a combination of news and style, with an annotated bibliography and a usable index, make Higgs' book well suited for courses on the history of American blacks or the postbellum South.

Higgs' news is the change and diversity of blacks' economic and (sketchily covered) social conditions during the postbellum era. This news is not that new, of course. Vernon Wharton, in *The Negro in Mississippi, 1865–1890*, emphasized the diversity of the black postbellum experience and the *evolution* of segregation, disfranchisement, and separate-but-equal schooling. Wharton even identified why the cartels of planters failed to reduce freedmen wages: particular planters so wanted labor that they broke ranks and offered a higher wage. Wharton also emphasized the great mobility of blacks within the South. Various scholars have told similar tales about other southern states, though not so movingly or so well. And C. Vann Woodward convincingly showed that *The Strange Career of Jim Crow* began late in the postbellum South. But, to judge from the reactions of contemporary college students, it is still news that anything changed, let alone improved, for American

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blacks before 1915, or even before 1941 or 1954. Hence, Higgs' news is most welcome.

Higgs' goals are to measure and explain the pattern of blacks' incomes between 1865 and 1915. He first gently introduces economists' approaches to the generation of income (from provision of valuable inputs), migration (for betterment), and racial discrimination (costly to blacks *and* whites). These are the focal points of his survey, for blacks strove to own more inputs (through saving) and more valuable inputs (through migration and education), while public and (less effective) private discrimination retarded the effectiveness of blacks' efforts. In subsequent chapters, Higgs employs his intuition or his and others' analyses (particularly that of W. E. B. DuBois) to interpret published aggregate data about the black demographic experience, changing occupations and work environment, and rising consumption. He concludes with an accurate summary of the black postbellum experience: blacks faced many unique obstacles, but labored so hard that they managed to prosper.

Higgs recognizes that the economic prosperity of blacks came slowly: slower than it did for southern poor whites or west coast orientals. Higgs attributes the slower progress of blacks to disfranchisement, which blocked blacks from public education and protection; but he fairly notes that this explanation seems in conflict with the economic record of Japanese-Americans.

In sum, Higgs swiftly interprets black Americans' economic past, 1865–1915, with the help of some others, some algebra, and some economic theory. He invariably shows the worth of algebra and frequently shows the worth of simple economics. But he shows neither in an obtrusive manner, putting only well-expressed results in his text; the nitty-gritty appears in clear but skippable technical appendixes. The end result is a fast and though-provoking read.

JOSEPH D. REID, JR
Southern Methodist University

JOEL SCHOR. *Henry Highland Garnet: A Voice of Black Radicalism in the Nineteenth Century.* (Contributions in American History, number 54.) Westport, Conn.: Greenwood Press. 1977. Pp. xii, 250. $15.95.

During the antebellum period Henry Highland Garnet was one of the most dedicated, militant, and active abolitionists. He was involved in temperance work, politics, the free-produce movement, journalism, international peace, and emigrationism. Yet, until recently, he was one of the least known of the black antislavery advocates. This low historical profile can be attributed in part to the aura of controversy that surrounded Garnet. Was he a revolutionary or simply one who used more militant rhetoric than his contemporaries? Did he do the bidding of the antiblack American Colonization Society or was he a true emigrationist? After 1850 did the ideas he espoused in his 1843 *Address to the Slaves of the United States of America* significantly influence American abolitionism? An understanding of the man and his role requires that such questions be answered. But most historians have taken a narrow view of his contributions. Consequently, they have answered these queries inadequately and have created a one-dimensional historical portrait.

Fortunately, Joel Schor succeeds in making Garnet a multi-dimensional presence. Through Garnet he demonstrates that black abolitionists were not a homogeneous group and that a categorization into camps of pessimists and optimists is too simplistic. Although the narrative primarily covers the years 1840 through 1865, his first chapter comes from an earlier period and depicts the radicalization of Garnet. It is a brief, concise statement of the education of a bright, able, precocious fugitive. Moreover, informal influences are not neglected: the author reveals that confrontations with antiblack mobs, in his youth and later, were integral to the fiber of Garnet's radicalism. This is easily the best chapter in the book.

Unfortunately, the remaining chapters which depict Garnet as a leading ideologue, Liberty Party advocate, black suffrage activist, Republican Party supporter, and emigrationist do not maintain the strength and clarity of the first chapter. Schor's failure to answer several important questions reveals indifferent or careless scholarly attitudes. For example, chapter seven, entitled "The Black Republican," is unsatisfactory. It does not sufficiently address Garnet's Republican activity but rehashes his participation in the New York State Suffrage Association. The most promising issues, such as whether Frederick Douglass recruited Garnet into the party, remain unexplored. The question is relevant since the author writes of a Douglass/Garnet rapprochement on the latter's return to the United States. Secondly, the author's juxtaposition of Douglass' embarrassed shift to the Republicans in mid-August 1856 with Garnet's decision to join the party within the month but after the two had shared the lecture platform during July and August raises pertinent questions.

While Joel Schor's work supersedes existing monographs on Garnet, the quality of his work is uneven; his central ideas concerning Garnet's militance and intuitiveness about new antislavery options are repetitiously stated and seldom clearly evaluated. Sadly, Henry Highland Garnet's important contribution still awaits a substantive study and clear analysis.

HARRY A. REED
Michigan State University

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LOUIS R. HARLAN and RAYMOND W. SMOCK, editors. *The Booker T. Washington Papers*. Volume 5, *1899–1900*. Urbana: University of Illinois Press. 1976. Pp. xxviii, 747. $17.50.

Booker T. Washington believed that blacks would achieve racial independence and dignity in alliance with the forces of Southern capitalism. This volume of the Washington papers includes *The Future of the American Negro*, his first book, which popularized the guiding assumptions of his racial politics. The letters of 1899–1900, enriched by the editors' annotations, demonstrate the conflicts embedded in Washington's theory and provide important clues to the task of understanding the social impact of capitalism on race relations.

The man who looked to economic development to resolve the problems of race relations was drawn into politics. Despite Washington's ideology, mass politics was the principal tool of a people without property, and the views of Southern capitalists on the franchise were not so different from those of the planters. Even though the zenith of Populist and Republican politics had passed, Southern Democrats retained a healthy respect for their antagonists, as J. Morton Kousser's important *Shaping of Southern Politics* argues. For Democrats, disfranchisement was the prerequisite to Southern development. They would first secure the polity and then contemplate racial amelioration.

Mass politics was not critical in Washington's tactics. But black support for his leadership precluded political retrogression, at least for properties and educated blacks. In 1899 and 1900, Washington tried to use his personal acclaim, Tuskegee's steady growth, and new business allies to defend the franchise. His efforts were blunted by his social ideology and allies. The failure to avert Alabama disfranchisement in 1901 was foreshadowed by his promotion of the all-white Montgomery Race Convention in 1900. While Washington was widely praised, all but one of the "better whites" opposed black voting. (Former governor McCorkle of West Virginia urged property and educational restrictions.) In 1899, Washington, local blacks, Republicans, poor whites, and labor unions had successfully fought off new restrictions in Georgia. Washington's failure to enlist more devoted supporters of the franchise in Alabama reveals the secondary role of the ballot in his thinking. But his presence in the Jim Crow gallery at Montgomery seems gratuitous. His attempt to transform the setback into a "beginning" (p. 513) is unconvincing. While gaps in Washington's outgoing correspondence limit our understanding of his strategy, letters from William Baldwin and Henry C. Davis suggest that Washington had contemplated public opposition.

Railroad manager William Baldwin overcame Washington's doubts, the advice of his principal black Northern adviser T. Thomas Fortune, and other black and white supporters. Baldwin, who had reorganized Tuskegee Institute's finances and management, given Washington important Northern contacts, and intervened to win land grants for the school, emerged as Washington's leading strategist on Southern politics and race relations.

The most charitable, and probably accurate, explanation of Washington's decisions on the Race Convention was Alexander Walter's. The head of the Afro-American Council assumed Washington's support of the franchise was "but for your school" (p. 595). The developing differences among blacks were racial priorities shaped by social position, not by racial goals. As Washington identified the growth of Tuskegee with the progress of the race, he was forced to create a vehicle—the National Negro Business League—to rally the black community behind his leadership.

It is always difficult to calculate Washington's personal role in the development of black politics. As his power grew, the school politics of Black Belt Alabama migrated to other parts of the South, and to the North. But the volume's excellent biographical sketches of the beginning Tuskegee network—businessmen and would-be entrepreneurs, politicians, and journalists—suggest that Washington gave voice and direction to existing social developments in black life. The racial impact of the events of the late nineteenth century was not uniform. Walter Weare's excellent *Black Business in the New South* has shown that the new elite in Durham, North Carolina was not simply a response to segregation, disfranchisement, or Washington's advice. The letters of political leaders and the incidence of racial restrictions and violence alone cannot formulate the critical relationships among black and white businessmen, laborers, and farmers of the South. Before we can identify the particular social experiences and aspirations which produced the new urban black elites and politics, we will need to inform the traditional resources of black history with the broader perspectives and theories of the new work in Southern urban history.

JUDITH STEIN
City College,
City University of New York

GÖRAN RYSTAD. *Ambiguous Imperialism: American Foreign Policy and Domestic Politics at the Turn of the Century*. (Lund Studies in International History, number 6.) Stockholm: Esselte Studium. 1975. Pp. 365. 58 KR.

Göran Rystad, a Swedish scholar, has written an important new study on American imperialism at

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the end of the nineteenth century. Although *Ambiguous Imperialism* suffers from sloppy copy-editing and his own occasional problems with idiomatic English, Rystad's strenuously researched book should command the same kind of attention and standards of criticism as those written by seasoned scholars from the United States. If this book is an index to the health of American Studies in Sweden, it is encouraging, indeed.

Rystad knows the literature of the subject and lays it out at the beginning to describe his own purpose, which is mainly to analyze the relationship between U.S. foreign policy and American politics in the years 1898–1900. He is far more successful in showing how foreign policy affected politics than vice versa. The most original part of his book is his persuasive argument about the importance of the Puerto Rican tariff issue in pushing the Democrats toward a "radical" anti-imperialist stand (a characterization I would disagree with), throwing many Republicans on the defensive, and influencing how the "imperialism" versus "expansionism" debate was defined in the 1900 campaign.

Other important contributions include a useful, though not, I think, crucial analysis of the state of Indiana's handling of the imperialist issue and, especially, his unprecedentedly detailed examination of the political history of the issue throughout the election year of 1900. Rystad covers the period from preconvention maneuverings through the general election, Democrats and Republicans, all shades of anti-imperialist opinion—at both the state and national levels. And, it should also be added, often using sources never before successfully exploited.

There is a problem in his analysis, however. Although his conclusion that the imperialism issue was not profoundly important in the campaign of 1900 is generally convincing, Rystad has not added any insights beyond those already available. It is doubtful whether our understanding of this issue will be significantly increased until a diplomatic historian trained also in quantitative electoral analysis tackles it.

I would quibble with Rystad about some important matters of judgment. I think, for instance, that he underrates how much many Republicans defended the annexation of the Philippines on grounds that it was practical and "good business." I also question the judgment that the election caused the GOP to abandon annexationism as a policy: the evidence does not yet show that the most important Republicans in 1900 regarded "annexationism" *as* a "policy."

Ambiguous Imperialism, however, is an important book, excellent in showing how both Democrats and Republicans dealt with the imperialism issue in 1900, in depicting the writhing miseries of choice-ridden anti-imperialists, and also in again demonstrating the wisdom of Thomas B. ("Czar") Reed's devastating quip that Bryan would rather be wrong than president.

ROBERT L. BEISNER
American University

ARTHUR S. LINK, editor. *The Papers of Woodrow Wilson*. Volume 21, *1910*; volume 22, *1910–1911*. Princeton: Princeton University Press. 1976; 1977. Pp. xx, 644; xxii, 630. $25.00 each.

The five months covered in the first volume witnessed Wilson's spectacular transition from pedagogue to politician. The correspondence indicates that he became a genuine Progressive somewhat reluctantly and by force of circumstance. His patrician instincts recoiled from the noisy shirt-sleeved Progressivism of Bryan and his followers, he disliked Roosevelt, disapproved of labor unions, and deplored demagogic attacks on big business. His gubernatorial campaign got off to a poor start under the dubious auspices of Colonel George Harvey, an ally of big business, and a bevy of local Democratic bosses. After friends got him squared away on a Progressive course, however, he won easily.

The text consists primarily of campaign speeches—forty-nine delivered before enthusiastic audiences in nearly as many cities and towns over a five-week period. Since Wilson spoke extemporaneously or from notes, his remarks have been culled mostly from various newspapers with minor editorial corrections, but giving a fair rendition of style and content. Then there are numerous editorials, personal interviews, special articles, and a few addresses to out-of-state groups.

On the stump Wilson strove to appear, not as a party leader, since both parties were equally corrupt, but as a public-spirited citizen dedicated to redeeming the state from boss rule. Moreover, the paucity of Democrats made an appeal to independent and "insurgent" voters (mostly Republicans) essential. In the midst of the campaign he was dismissed from the presidency of Princeton University by a clique of hostile trustees, an action that hurt and angered him deeply. Before leaving Princeton, however, he obtained a change in the football rules which eliminated the "brutal features" of that sport and made it "now a game in which gentlemen can successfully engage" (22:5).

During his first five months in office, covered in the next volume, Wilson displayed strong executive leadership in dealing with the legislature on two vital issues. First, he was called upon to block the election to the United States Senate of his

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former patron, powerful Hudson County boss James Smith, Jr. Smith's refusal to withdraw precipitated a bitter controversy with charges of bad faith on both sides since Smith claimed a prior commitment which the governor rather lamely denied. Wilson considered the Progressive Democratic candidate, James E. Martine, a demagogue and was loath to support him; but when warned that a Smith victory would kill him politically, Wilson overcame his repugnance for his fellow Progressive and won over enough Smith henchmen to defeat the boss. The struggle attracted nationwide attention and enhanced Wilson's standing among Progressive candidates for the 1912 Democratic presidential nomination. He also manifested equal dexterity in getting his reform program through the legislature, button-holing reluctant members and mixing freely with the opposition. After a country club stag dinner with a number of unfriendly Republican state senators, the guests sang popular songs, danced zanily around the room with each other, and enjoyed "one unbroken romp" (22: 532).

Most informative at this time are twenty-six letters to Mary Allen Peck (Mrs. Hurlbut), then wintering in Bermuda where Wilson had met her some years before. In moments of weariness and at odd hours he poured forth his inmost thoughts and feelings, his frank impressions of people and events, and his deep concern for the lady's health and welfare in rather extravagant language that later became the basis for a nasty scandal. Congratulatory letters, many of little moment, take up much space in both volumes. The explanatory notes continue most helpful but giving the full name of everyone mentioned in passing seems excessive. The high standard of editorial excellence continues, although a few minor errors have escaped notice.

SEWARD W. LIVERMORE
Washington, D.C.

EUGENE P. TRANI and DAVID L. WILSON. *The Presidency of Warren G. Harding*. (American Presidency Series.) Lawrence: The Regents Press of Kansas. 1977. Pp. ix, 232. $12.00.

This is the sixth volume to appear in the American Presidency Series. The first five dealt with Washington, John Adams, Jefferson, Buchanan, and Taft. Now comes Warren Harding.

The account given by Eugene P. Trani and David L. Wilson is a familiar one. Harding was a small-town Ohio politician who brought both his liabilities and his assets to Washington. A conciliator yet a loyal party man, he possessed good intentions but limited presidential capabilities. He won the presidency in 1920 mainly as a reaction to Wilsonism and occupied the White House during two difficult transition years from World War to prosperous peace. His appointments were both good and bad. His normalcy program was essentially probusiness. Under him, taxes were decreased (especially for the wealthy), a budget reform act was passed, higher tariffs were instituted, the farmer was given only modest relief, the interests of labor were generally ignored, and the national debt was reduced. Yet, he also freed jailed wartime dissenters like Eugene Debs and favored a department of public welfare, antilynching legislation, maternity and child care aid, federal highway construction, and an eight-hour day in the steel industry. In foreign affairs, his administration held the Washington Disarmament Conference, "normalized" relations with our European wartime enemies, and cemented better relations with Latin America.

Harding saw much of his program enacted before his death. But he achieved it at tremendous political cost. The executive was constantly at odds with a farm-bloc-ridden rebellious Congress, and the Republican Party was almost left stranded by the disastrous elections of 1922. In the end, however, it was not so much Harding's policies as his undiscerning judgment of men and his failure to act decisively regarding political scandals that ruined his presidential reputation.

The authors treat this story with considerable balance and detachment. In the brief space allowed them (192 pages of text), they adequately cover the administration in all its aspects. They are to be commended for keeping Harding's personal peccadilloes and the scandals in the proper perspective. Their style is straightforward and clear. The index is useful and the bibliographical essay is excellent.

The authors' final judgment of Harding as a poor president (which few will dispute) raises a question, not because of what is said but because of the procedure by which it was reached. According to the editors' preface, this series is intended to give the reading public "interesting, scholarly assessments of the various presidential administrations [drawing] upon the best in pertinent secondary literature." Obviously, the difficulty with this method is that final conclusions cannot be sounder, from a scholarly viewpoint, than the secondary sources from which they are derived. In Harding's case, this presents a real dilemma since almost all of the secondary literature is either hopelessly biased or does not rest on primary sources or manuscripts. A "scholarly" assessment of Harding is therefore not currently possible in the same way that it is for the other five presidents in the series. The necessary monographic building

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blocks, utilizing the Harding Papers, are simply not in existence, although a growing number of Ph.D. dissertations are becoming available. As indicated in their footnotes, the authors were forced to rely excessively on only a handful of recent analyses which had used the Harding sources (including my own *The Harding Era* and *The Politics of Normalcy*). Not being required or expected to deal with primary materials themselves, the authors managed a skillful blend of these secondary writings and then opted for the traditional conclusion.

This type of book was probably attempted too soon. To be consistent, if the historians' own presidential polls were followed, Harding should have come at the end and not near the beginning of this series. But, then, considering poor Warren's last-place finishes, he might as well enjoy the present small favor.

ROBERT K. MURRAY
Pennsylvania State University

HIRAM M. DRACHE. *Beyond the Furrow: Some Keys to Successful Farming in the Twentieth Century*. Danville, Ill.: Interstate Printers and Publishers. 1976. Pp. xiv, 551. $9.95.

Most historians readily acknowledge the vital contributions of progressive farmers to American agriculture. In this lengthy study, Hiram M. Drache—author, professor, farmer—elaborates on the theme and carries it a step further. He concludes that the farm industry's future lies with today's innovators, that unparalleled opportunities exist for skilled, creative husbandmen, and that agriculture, with large-scale farmers at the helm, is the growth industry of the present and the future.

Drache develops his ideas by detailing the achievements of several dozen pacesetters who from the late nineteenth century to 1972 revolutionized farming practices in Minnesota, Iowa, the Dakotas, Montana, and southern Manitoba. While thorough treatment is accorded the critical function of farm technology, the author emphasizes the human element as the key to farm success. Managerial ability, optimism amidst adversity, openness to ideas and experimentation, a zest for challenge, are among the traits common to Drache's hard-driving innovators, not to mention keen marketing skills and a talent for fund-raising. No matter the product or region, enterprising farm operators, he asserts, have found and will continue to discover ways to cut costs, improve quality, up production, and to prosper.

Nine of the book's fourteen chapters focus wholly or in part on the Red River Valley, the setting for two previous works by the author. Extended treatment is given Thomas Campbell, the wheat king from Montana, and Iowan Roswell Garst, the super-promoter of hybrid corn, commercial fertilizers, and novel cattle-feeding methods. A perceptive final chapter analyzes the farmer's banker, a rare figure in agricultural histories. Certainly the trend to highly capitalized, integrated farming is no revelation to anyone, but Drache documents the process with a wealth of personal detail no previous study can match. Nearly two hundred personal interviews provide the grist for this tribute to big scale agriculture. (A projected second volume will analyze contemporary innovators and the agribusiness industry.)

And what of the family farm in an era of computerized agriculture? As in the past, Drache argues, it will be the progressive family-oriented farm that can best furnish the human commitment so essential to farm success.

GERALD PRESCOTT
California State University, Northridge

GARIN BURBANK. *When Farmers Voted Red: The Gospel of Socialism in the Oklahoma Countryside, 1910–1924*. (Contributions in American History, number 53.) Westport, Conn.: Greenwood Press. 1976. Pp. xvi, 224. $13.95.

Historical theories about socialist movements have largely centered on urban proletariats, despite the imposing anomaly represented by modern China. Oklahoma radicalism stands as a home-grown anomaly, one that emerged during the early Socialist organizing effort in America and developed into one of the party's most elaborate regional expressions. As Garin Burbank shows through well-organized maps, the Socialist ticket received, in the multi-party Oklahoma political environment of 1914, no less than 25 percent of the vote in 33 of the state's 77 counties and 15 to 25 percent in an additional 19 counties. An interesting problem for historians: was such visible radicalism across two-thirds of a rural state simply a product of "hard times," or did insurgent spokesmen develop some cultural or organizational method of creating a mass socialist party?

Unfortunately, this new study offers a narrow and unpersuasive answer. At the outset, the author (with some apparent equivocation) seems to reject cursory modes of economic determinism. Voting patterns in counties containing high rates of tenancy prove only that while radicalism flourished in some, it floundered badly in others. Similarly, rising commodity prices correspond with a downturn in the party's fortunes, but, inconveniently, correspond also with rising "frustration" among tenants. Finally, the postwar agricultural depres-

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sion—when times were unrelievedly "hard"—coincided with the party's collapse.

But in terms of possible organizational roots, the author makes only glancing references to the radically class-conscious Oklahoma Renters Union, and he virtually ignores the much larger Farmers Union. As for the Socialist Party itself, its organizers are viewed as essentially irrelevant to the radical awakening, being "occupationally inclined" to overvalue their own efforts.

How did it all happen then? The operative ingredient, we are told, was "old-time religion." Through this device, radicals found a way to appeal to a "proven morality, to a sense of righteous conviction." This conclusion seems hard to credit—given the religiosity of thousands of Oklahoma farmers who remained politically orthodox, not to mention the prominent role played by rural Protestant ministers in the Ku Klux Klan movement (which socialist constituencies consistently opposed and business constituencies consistently supported). Indeed, tension between believing Christians and skeptics was a recurrent problem within the Socialist Party in Oklahoma, as elsewhere. For socialists, fervent religious beliefs have traditionally constituted an organizing hurdle.

If socialist organizers sometimes couched their culturally embattled ideas in culturally sanctioned Biblical metaphors, the question remains: how did they manage to recruit the farmers to hear them in the first place? It is clear that ex-Populist organizers who emigrated from Kansas and Texas not only became quite active in Oklahoma socialism, but played a central role in the creation of the mass-based Oklahoma Farmers Union. Indeed, the rise of the "non-political" Union broadly paralleled the rise of the radical party. These and other ideological and organizational conjunctions would seem to merit further attention before we can fathom that volatile moment "when farmers voted red."

LAWRENCE GOODWYN
Duke University

PAULA S. FASS. *The Damned and the Beautiful: American Youth in the 1920's*. Oxford University Press. 1977. Pp. xii, 497. $15.95.

Were young Americans in the twenties true moral rebels against a stifling bourgeois culture? Or were they mere apprentice Babbitts, asking no more of that culture than how to get ahead in it? Rather than answer such questions with the historian's usual tepid eclecticism, Paula Fass has taken a bolder, more intellectually demanding, and more imaginative course. Drawing both upon an abundant sociological literature and upon the lively collegiate press of the twenties (*Barnard Bulletin*, *Cornell Sun*, *Ohio State Lantern*, and the like), she has synthesized these seemingly unblendable materials into a fresh, persuasive hypothesis as to what really happened to youth during America's twenties.

Raised within an American family structure which was itself undergoing transformation toward more personalized and affectionate internal relationships, young people found this kind of upbringing considerably out of phase with the bureaucratic and impersonal demands of an equally changing capitalist adult society. Into this growing gap between family and society, Fass argues, stepped the peer group, to generalize and socialize the young person's way of living. Unlike some other scholars she cites, however, Fass does not believe that the peer group merely housebroke the individual into the norms of the corporate whole. Quite the contrary. Peer grouping gave young people an intermediate culture, neither family-bound nor society-controlled. Peer pressures were conformist, sometimes cruelly so; but the values such pressures reinforced were not quite identical with those of conventional culture. If "youth rarely provoked adult authority on the high ground of theory"—i.e., by becoming in the formal and political sense "radical"—it chipped away at that authority in a never-ending game of ridicule, evasion, and sabotage, clearing the way for the establishment of new norms of its own.

College youths especially, as they postponed induction into the working world much longer than their noncollege contemporaries were able to, found a kind of leverage upon the society as a whole, and wrested from that society the unwilling compliment of imitation. Collegians were a minority of the population, but they were a strategically placed (and media-dramatized) minority. Also, there were many more of them than there had ever been in America before. College students knew they would in due course become members of the larger society, and they consciously prepared themselves for its disciplines; in this sense perhaps they were incipient Babbitts. For the time being, however, as they moved within a transitional milieu they could call their own, these youths created moral and cultural precedents which would carry over into the adult world. Politically, Fass contends, these youths were conservative but not traditionalist—a most important distinction. "They did not agitate for change," she sums up, but they did "oppose . . . attempts to suppress the possibility of change."

It is an attractive hypothesis, although it contains a few pitfalls. The campus newspapers, in particular, constitute a rich—and previously all but untapped—historical source, which Fass has mined with an unerring eye for the apt quotation.

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She also notes the ever present threat of administrative censorship. But that same awareness must have prompted college editors in the twenties to voice their peer-group aspirations with discretion; one wonders, therefore, whether or not some of those aspirations were filtered out of the record entirely. Moreover, much of the college press in those days was controlled by the fraternities, which constituted something of an elite within an elite; the "collegiate" values it championed, as indeed the author acknowledges, often amounted to no more than a specialized mode of conspicuous consumption, firmly tied to such off-campus forces as the clothing industry.

Outside the orbit of the beautiful people in their white-pillared sorority and fraternity houses were the kinds of students this book treats as having been "marginal" to college life, but they were the majority. Is it not possible that these unaffiliated young people—"barbarians," in campus jargon—were quietly and individually doing more in college that would count in the long run of history than were their "Greek" contemporaries? It is a measure of the book's analytical excellence that it thus challenges the reader to carry still further its dialectic on the youth culture of the twenties.

PAUL A. CARTER
University of Arizona

CHARLES A. JELLISON. *Tomatoes Were Cheaper: Tales from the Thirties*. Syracuse University Press. 1977. Pp. ix, 240. $10.95.

This is a book toward which I was favorably disposed at the outset for several reasons. First, it is about the 1930s, and I would be hard put to name a book about the 1930s that I do not like. Second, it has a fascinating chapter, "Me and Paul," about Dizzy and Paul Dean and the St. Louis Cardinals' Gashouse Gang, who beat the Detroit Tigers in 1934 in the most riotous seven games in World Series baseball history, and I have been a St. Louis Cardinal baseball fan for more than fifty years. Finally, the author has some nice things to say about one of my books, and I am as susceptible to flattery as the next fellow.

The book is not a history of the 1930s. Instead, says the author, "I have wandered nostalgically through the shoals and narrows of that depression-ravaged decade" with the purpose of recapturing "something of the flavor of American life as it was during those years." The result is a series of vignettes, including pieces on Judge Crater's disappearance, the Bonus Marchers, a kidnapping-murder in San Jose, California, a twelve year old boy lost on a mountain in Maine, some simple-minded hoodlums in Maine trying to emulate John Dillinger, and the antics of the Dean brothers. There are also three pieces that may be described as political. One concerns the effort in the Senate in 1935 to get approval for United States membership in the World Court, another is on the election of 1936, and the third summarizes the Dies Committee investigation of the WPA Theatre project.

But the book is flawed. It lacks footnoting, bibliography, and index. The pieces are arranged chronologically, but there is no discernible rationale for their selection or for the emphasis given each. There is something disconcerting about turning from the Gashouse Gang story of 1934 to the story of the World Court in 1935. For a book that is subtitled "Tales from the Thirties," the political essays seem strangely out of place. Why is the kidnap-murder in San Jose somehow more representative of the decade than the Lindbergh kidnapping-murder? Why does it take forty pages to tell the story of the Bonus Marchers but only eleven pages to relate the story of the World Court, and sixteen pages to summarize Roosevelt's re-election in 1936?

The crucial question would seem to be does this book add anything significant to the bibliography on the New Deal and Great Depression, a bibliography that is already enormous and continues to grow like the national debt? The answer must surely be negative. But the pieces are well written. And if one's purpose is to be entertained, then perhaps this book is justified. If the purpose is to read serious history, then one will have to look elsewhere.

GEORGE WOLFSKILL
University of Texas, Arlington

JAMES HOOPES. *Van Wyck Brooks: In Search of American Culture*. Amherst: University of Massachusetts Press. 1977. Pp. xviii, 346. $15.00.

James Hoopes' *Van Wyck Brooks* is the first full-length biography of one of the most influential literary and social critics in the twentieth century. Other shorter studies are available, chiefly William Wasserstrom's analytical *The Legacy of Van Wyck Brooks* (1971) and James R. Vitelli's *Van Wyck Brooks* (1969). Hoopes' exhaustive chronological account of the literary historian's life and detailed analysis of the critic's personality and writing surpasses these books in scope. This biography also contains insights and information unavailable in Brooks' three-volume autobiography, which the author judiciously avoids as his primary reference source.

The biography is partly based on the valuable

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Van Wyck Brooks Collection at the University of Pennsylvania, where Hoopes has uncovered unpublished diaries, manuscripts, and letters. Gladys Brooks, the critic's widow, and Brooks' two sons cooperated in the project by providing additional material. Numerous other collections were consulted, including the Randolph Bourne, Newton Arvin, and Waldo Frank papers. Hoopes has seemingly read practically everything Brooks wrote, a formidable task considering the critic's output, and has studied the medical files on Brooks' mental illness. The result is an extraordinarily well-researched biography, rich in new knowledge and perception of its subject.

Hoopes portrays Brooks as a writer plagued by fears and contradictory beliefs. Brooks' insecurity and values partly stemmed from childhood. His antipathy towards commercialism derived from being born into a family of declining economic status and observing his father lose a fortune speculating in mines and working perfunctorily as a clerk in a New York brokerage house. His brother Ames, a frustrated poet turned lawyer, committed suicide in 1931 after losing money on the stock exchange. Brooks thought American materialism antithetical to the arts, but he yearned for social status and enough money to sustain his family comfortably in suburban Connecticut. Known as a cultural rebel who revolted against the genteel literary tradition, Brooks was actually conservative in dress and manners, and modeled his life on the European man of letters. His ideal of a new indigenous American culture derived from his respect for European culture where art and tradition fused. Although Brooks believed that art had a social purpose, he shunned political involvement. The conflict between his idealism for the arts and the reality of American life perturbed him over the years. In his personal life he worried about his manliness and had doubts concerning his talent as a literary critic. His own awareness of the weaknesses in his books on Mark Twain and Henry James contributed to his nervous breakdown in 1926, but even before this time he showed manic-depressive symptoms. The chapter on Brooks' five-year "season in hell" in hospitals and sanitariums is sensitively written, revealing the critic's mental suffering and futile attempts to commit suicide.

Hoopes agrees with other assessments of Brooks' work that his writing became more affirmative after his recovery in 1931. He points out, however, that the earlier work forecasted the author's need to celebrate rather than denigrate America's literary past. Hoopes diligently traces the major influences on Brooks' thought and analyzes the books and articles, including his pieces for *The Seven Arts* and *Freeman* magazines. The observations on his writing are acute, although not always new, for he shares with the new critics a tendency to disparage Brooks' sociohistorical approach to literature. Nor does he fully evaluate his subject's enormous contribution to American literary history, particularly Brooks' rediscovery of forgotten writers. Only in the last chapters is Brooks presented as a distinguished and honored man of letters. The overall tone is one of tragedy, perhaps even failure. Brooks' search for American culture was marred by psychological insecurities, financial worries, rejection by his colleagues, and an inability to deal critically with certain literary subjects. Hoopes convincingly chronicles Brooks' struggle to deal with these problems.

ARTHUR FRANK WERTHEIM
University of Southern California

SIDNEY HYMAN. *Marriner S. Eccles: Private Entrepreneur and Public Servant*. Foreword by G. L. BACH. Stanford: Graduate School of Business, Stanford University. 1976. Pp. xviii, 456. $15.00.

Marriner Eccles, so the foreword to this biography notes, was among the few men to "make a difference in the course of human history." As banker and businessman, he was an organizational innovator. And during his seventeen years on the Federal Reserve Board (1934–1951), he was a central figure in shaping the banking reforms of the 1930s, promoting the Keynesian revolution in fiscal policy, and sparking the Federal Reserve's successful revolt against Treasury control. He also encapsulated in himself much of the coming accommodation between corporate capitalism and the liberal state. In the 1930s he urged a mix of public and private action remarkably similar to what eventually took shape and can therefore be seen as a spearhead for the kind of business modernists who subsequently organized the Committee for Economic Development, adapted Keynesianism to corporate needs, and formulated the partnership ideas of the 1950s and 1960s.

The need for a study of Eccles has long been apparent; and in this offering by Sidney Hyman, who helped prepare Eccles' *Beckoning Frontiers* in 1951, interested scholars will find much of value. Although closer to journalistic biography or assisted memoirs than to academic history, the book does draw heavily on the primary documentation in the Eccles papers, supplements this with solid secondary accounts and firsthand knowledge of the Washington scene, and offers plausible explanations for the course taken by Eccles' career. It also reflects Hyman's considerable skills as a narrator, organizer, and political commentator. Its weaknesses, from the standpoint of the historical scholar, are its over-reliance on the Eccles collec-

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tion, its predisposition to accept Eccles' point of view, and its innocence of the scholarly debates that have recently swirled about the New and Fair Deal policies. The author does not draw upon the literature that these have generated or seek to relate his material to them.

On the New Deal years, Hyman's account contains relatively little that is new. But it does provide much fresh material on the rise of the Eccles family and fortune, the influences that led the builder of a banking empire to Keynesian prescriptions before Keynes had arrived at them, the long and often bitter struggle between Federal Reserve and Treasury officials, and the making of wartime and postwar economic policy. Also fascinating are the chapters on Eccles' return to business life in the 1950s and his subsequent involvement in such causes as population control, the management of economic growth, and peace in Vietnam. The whole adds up to a substantial contribution. And while there are occasional slips (Hyman, for example, has the John Birch Society reacting with outrage before the society was formed), these are not such as to undermine confidence in the author's grasp of the historical context. The book can be read with profit by scholars and intelligent laymen alike.

ELLIS W. HAWLEY
University of Iowa

FREDERICK C. ADAMS. *Economic Diplomacy: The Export-Import Bank and American Foreign Policy, 1934–1939*. Columbia: University of Missouri Press. 1976. Pp. xi, 289. $12.50.

This work contributes such valuable information about the formation and function of the Export-Import Bank in the last half of the 1930s that the cutoff date of 1939 leaves the interested reader wondering why Frederick C. Adams did not carry his analysis through American entrance into the Second World War. Such coverage would have been more valuable than the time and effort spent in the first two chapters summarizing economic relations of the United States during the 1920s and the first years of the Great Depression. These introductory chapters offer little that is new in the way of interpretation and ignore some of the latest work on Herbert Hoover's economic diplomacy between 1920 and 1933, as well as several new perspectives on the period that have appeared in articles and dissertations by Ellis W. Hawley, Michael J. Hogan, William B. Kelly, Jr., Robert H. Van Meter, Jr., Nelson Eugene Woodard, and Robert Neal Seidel.

Once Adams begins to concentrate on the Export-Import Bank, however, the book assumes important dimensions. Of particular interest is his description of the differences of opinion between George N. Peek, the first president of the lending agency, and the State Department over foreign trade policy. Adams also notes that from the beginning the bank served the dual purpose of offering support to American exporters and giving FDR's administration the means for achieving certain foreign policy objectives. He traces in detail how and when these two goals reinforced or conflicted with one another, concluding somewhat ambivalently that government "policy makers were not directly controlled by interest groups," although "over the long haul they may well have been dependent upon them" (p. 257).

Part of this ambivalence stems from Adams' reluctance to admit how intertwined business and government leaders' roles became, for example, in formulating economic policy toward Japan. Irvine H. Anderson, Jr. has ably demonstrated this complex interrelationship with respect to the use of embargoes against the Japanese. Adams' sources and research are more heavily weighted on the side of government officials rather than on businessmen, interest groups, or large corporations. Nonetheless, the work is well written and provides insights into the operations of the Export-Import Bank that are unavailable elsewhere.

JOAN HOFF WILSON
Arizona State University

CHARLES H. MARTIN. *The Angelo Herndon Case and Southern Justice*. Baton Rouge: Louisiana State University Press. 1976. Pp. xv, 234. $10.95.

Legal history is clearly too important to be left to the lawyers, or even to constitutional historians, if that term is narrowly construed. A constitutional issue was resolved, to be sure, in the Supreme Court's ruling in 1937 that the Georgia insurrection statute of 1866 violated the Fourteenth Amendment. More important, however, Charles H. Martin uses the Angelo Herndon case to open the window on southern social attitudes in the 1930s. In Atlanta in the Great Depression, fear of the crumbling color line joined fear of the unemployed in a business-oriented city. When the chief organizer of the unemployed, Angelo Herndon, went to his post-office box, he was arrested for receiving Communist mail. A Communist Party member, Herndon was only nineteen years old with an elementary education, not much more sophisticated than the Scottsboro Boys a few hundred miles to the west in a more celebrated trial in the same period. Herndon was fortunate, however, because his lawyers were the able, though inexperienced local black lawyer, Benjamin J. Davis, Jr.,

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in the lower court and the blue-ribbon New York lawyer Whitney N. Seymour on appeal. Because Herndon asked at once for defense funds from the International Legal Defense, the NAACP stayed clear, and there was none of the divisive wrangling over defense strategy that marred the Scottsboro case. Furthermore, the ILD showed unexpected flexibility in giving the conservative Seymour a free hand in appeal strategy when he demanded it. This book uses effectively the manuscripts of Herndon's defense, as well as local archives, newspapers, and interviews. Martin writes of Communists in the nonaccusatory tone of the post-Cold-War generation, but without evidence of special pleading.

This book tells us little of Herndon himself, who played his role of pawn with impenetrable solidarity. Though it provides some insight into radicalism in the thirties, its chief contribution is the reminder that Atlanta officialdom and the Georgia judiciary of the period neither fulfilled the New South dream nor foreshadowed the city "too busy to hate." The Herndon case appropriately belonged in the city that had spawned the race riot of 1906, the Leo Frank case, the revived Ku Klux Klan, and the Black Shirts, and which a few years later would be agog over rumors of the Eleanor Clubs. But Atlanta also harbored President Harvey Cox of Emory, who defended his faculty against a witch hunt after two professors testified for the defense; young C. Vann Woodward, who strove for harmony between liberals and radicals on the defense committee; and Atlanta black leaders who looked behind the Red label to see in Herndon a brother in need.

LOUIS R. HARLAN
University of Maryland, College Park

CONSTANCE ASHTON MYERS. *The Prophet's Army: Trotskyists in America, 1928–1941*. (Contributions in American History, number 56.) Westport, Conn.: Greenwood Press. 1977. Pp. xiii, 281. $16.95.

The history of American Communism is a record of struggles over doctrines imported from abroad and advocated by jealous and quarrelsome spokesmen at home, "leaders" whose personality clashes, conflicting ambitions, and ideological differences inhibited the growth of a tiny organization. The story of Trotskyism is an important part of the factional convolutions which both damaged and symbolized the Communist effort in the United States.

When Leon Trotsky lost his struggle with Joseph Stalin and was expelled from the Soviet party and forced into exile, small bands of followers rose to his support within the struggling Communist groups in Western nations. These Trotskyists responded to the leader's argument that Stalin in power had created a "degenerate" bureaucratic system which was both suffocating and anti-Leninist, a Communism which would not spearhead the world revolution without which socialism's triumph could not be complete even in one country. It was in America that the largest group of Trotsky supporters would emerge, but even this number would remain pitifully small, ranging from less than two hundred in 1928 to one or two thousand in later years.

Constance Ashton Myers' *The Prophet's Army* chronicles the early and most important years of Trotskyism in America from 1928 to 1941. Based on a wide reading of memoirs of movement notables, newspapers and journals from different segments of American Communism, interviews and some fragmentary collections of available papers, this book adds to our understanding of the fraternal chaos which marked the American Left in what would seem to have been its time of opportunity, the decade of the Depression.

The author describes how Trotskyism, itself a Communist faction, went through an agonizing process of fusion and factionalism in its formative years. Always a marginal group ignored by mainstream politicians, as well as by the "masses" they sought to influence, Trotskyists in America followed the ideological directives of their exiled leader. At first Trotsky insisted that his followers act merely as a propaganda group directed at purifying the Communist Party, and so the Communist League of America (Opposition) was born. When that effort was abandoned in 1934, the American disciples responded to Trotsky's new directive: fusion with other socialist groups, a technique known as the French Turn. First, the "Musteites"—followers of radical-pacifist A. J. Muste, who had organized an American Workers Party—were the targets of fusion. When the Trotskyists had joined (and dominated) that organization, the resulting Workers Party was folded into the larger Socialist Party of America. By the end of 1937, in another tactical change, Leon Trotsky encouraged his followers to provoke their own expulsion from the Socialist Party and create the Socialist Workers Party.

But the fatal sectarianism which marked even the efforts at fusion soon poisoned the atmosphere of the new SWP. Hurling favorite epithets at one another—"splitter," "adventurist," "traitor," "petty bourgeois"—Trotskyists attacked each other and several of the most articulate and dynamic members of the sect were forced out. This time, prophet Trotsky did not favor the expulsions, but his chief strategist in America, James P. Can-

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non, insisted on them in the interest of his own position of power.

The reader occasionally may lose the way as Myers traces this convoluted story of internecine struggles; she is not entirely successful in painting a clear picture of the source of some of the splinters. What might have helped is a more detailed exposition and analysis of mainstream Communism's twisting party line, particularly the impact of the Popular Front on the fortunes of the Communist Party in America and its paradoxical growth during the high tide of the New Deal.

But if her narrative demands particularly close attention, it is nonetheless rewarding. Myers' study fills a gap in the literature on the American Communist movement.

DAVID H. BENNETT
Syracuse University

HERBERT S. PARMET. *The Democrats: The Years After FDR*. New York: Macmillan Publishing Company. 1976. Pp. xi, 371. $12.95.

This book ably relates the public ideology of the Democratic Party from Harry Truman to Lyndon Johnson. Herbert Parmet has an engaging style of writing and analytically treats important figures and events. If the reader is seeking a book informed by the last generation of social science literature on the political process, he will not find it in Parmet's study. There is virtually nothing of secular voting trends, repeat interviewing of voters, or in general the exact quantitative work so familiarly assumed by political scientists. As a result, the book is by no means a synthesis of recent work on the Democratic Party. It excels at insights about major politicians and individual elections.

The author is nicely aware of the strengths and weaknesses of the Democrats in these years. The party, he implies, was too conventional to justify the faith that humanitarians frequently placed in it. But it reached high drama and worth in moments like the fight over the civil rights platform plank of 1948. John Kennedy receives an excellent treatment, the most balanced I have ever seen. The effort to rehabilitate the Robert Kennedy of the 1968 primaries fails, I believe; conversely, the harsh tone toward Eugene McCarthy (shrewdly compared to Adlai Stevenson) is scarcely justified. McCarthy rarely explained his actions, yet Parmet judges him by measures of standard politics. Some elections receive detailed treatment; other important ones, like that of 1952, are all but ignored. Some chapters, like that on 1968, are poorly written and unworthy of an author who generally writes well.

This is a book by an author who has opinions and invites the reader to agree or disagree; the tone is moral. When Parmet appraises individual liberals or conservatives in some detail, he is usually critical and thoughtful. But when figures are discussed more briefly there is a marked tendency to be pejorative about conservatives and to praise liberals. This mode, of course, does not reflect the complicated world of American politics.

DAVID BURNER
State University of New York, Stony Brook

JIM LESTER. *A Man for Arkansas: Sid McMath and the Southern Reform Tradition*. Little Rock, Ark.: Rose Publishing Company. 1976. Pp. 303. $9.95.

Elected governor of Arkansas in the rosy afterglow of World War II, the youthful Sidney McMath seemed on his way to a major political career. He could have been the first to break Jefferson Davis' record as the only Arkansas governor elected to three terms. He almost ousted John McClellan from his secure seat in the United States Senate. He was mentioned as a possible Democratic national chairman and vice-presidential nominee.

McMath had the classic credentials of a neo-Populist Southern politician: a log-cabin birthplace, humble origins, a boyhood struggle to help his family by shining shoes and delivering newspapers, a high school reputation for oratory, a valid claim to having worked his way through the state university by washing dishes and boxing. He embellished these credentials by service as a Marine Corps lieutenant colonel in World War II, entitled to wear the Legion of Merit. McMath attracted statewide notice when he led local veterans' groups in ousting a corrupt, long-entrenched political machine in Hot Springs. By the age of thirty-six he had been elected governor by those who hoped that he would spread the reform impetus to the entire state, pave its muddy roads, and educate its populace.

In his two terms from 1949 to 1953, McMath made progress toward improving highways, medical services, and educational facilities, renewed his state's courtship of industry, and risked the enmity of powerful corporations by advocating rural electric cooperatives. He also defied Arkansas Dixiecrats, stuck by the embattled Harry Truman, refused to take part in an anti-Communist witchhunt at the state university, and counseled Southerners to make racial change before federal troops arrived.

Arkansas in the early 1950s was not ripe for a political leader who was racially moderate, reform-minded, loyal to the national Democratic party, and cool to McCarthyism. But as Jim Lester points out in his study, McMath was a political

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casualty, not only of the times, but of his own poor judgment in condoning the use of the Arkansas Highway Department as a siphon for contributions to his political campaign. Revelations of political corruption tainted McMath's reform image and, coupled with his refusal to enter into the demagogic spirit of the Dixiecrats and McCarthyites, cut short his political career.

Like his Alabama contemporary, James E. "Big Jim" Folsom, who also rose from the subsoil of poverty to the governorship, McMath peaked twenty years too soon. Both charismatic neo-Populists, both beset by personal problems (Folsom was a victim of alcohol, McMath's wife shot and killed her husband's alcoholic father), both succeeded by protégés who abandoned moderation for fire-eating (Orval Faubus and George Wallace), McMath and Folsom seem cut from a strikingly similar pattern: flawed "might-have-beens" in the politics of the South and nation. Both, however, deserve attention from historians. Lester has made a useful and diligent, albeit woodenly written, contribution to the study of one of those advocates of racial moderation whom the South discarded.

VIRGINIA V. HAMILTON
University of Alabama, Birmingham

GERALD T. DUNNE. *Hugo Black and the Judicial Revolution*. New York: Simon and Schuster. 1977. Pp. 492. $12.50.

This book is the first full biography of Hugo LaFayette Black, a powerful New Deal senator and giant in the history of the United States Supreme Court. Well written by the author of a splendid biography of Justice Story, the work appears aimed at a general audience as it traces Black's origins in rural Alabama, his early manhood as a scrapping Birmingham lawyer, and his career as a populist senator from Alabama, responsible for major federal controls on public utilities and minimum wages and hours. Over half the book is devoted to Black's leadership as a justice, from 1937 to 1971, in transforming the role of law and the Supreme Court in American life.

The study is conceived as an "institutional history in the idiom of personal biography," but it is more successful in describing Black's personality and public performance than in linking the two. The portrait of Black's personality, "elusive as quicksilver," is filled with insight, gracious prose, and narrative power. The great debate between Justices Black and Frankfurter, Benthamite-puritan and professor-priest, is skillfully handled, even at the price of neglecting group relations on the high court. Dunne has few rivals in succinct summaries of complex cases or arresting vignettes. His admiration of Black does not cloud the paradoxes of the justice's quest for a jurisprudence of certainty. Nor does he pull many punches from Black's lower sides, e.g., aggressive tactics as a trial lawyer, alliances of political convenience, and overzealousness as a senatorial inquisitor against the "rattlesnakes" in big business, which presaged witch-hunting to come. Wrong but unrepentant is Dunne's judgment of Black's performance in *Korematsu* v. *United States*, upholding the relocation of Japanese-Americans in World War II, and of his icy silence, even to President Roosevelt, about his former membership in the Ku Klux Klan to save his nomination to the supreme bench.

Several flaws mar this dramatic story. The most serious handicap is its heavy reliance on secondary sources. The justice's son reluctantly obeyed Black's orders to burn his judicial papers. Contrary to dust-jacket claims, fresh materials are limited to interviews, published family writings, and collateral papers of other justices, some not fully exploited. Hence, this account of the judicial revolution is richer at the seeding in the courts of Hughes, Stone, and Vinson than during its flowering in the court of Warren, when the focus subtly shifts from protagonist to institution. Other handicaps are of the author's own making. Few cases are probed in depth, except for *Brown* v. *Board of Education*, in which Black's role remains obscure. There are many errors of fact, some borrowed and some new, regarding events, proper names, and bibliography. Interpretatively, Dunne tries to squeeze the desegregation cases into the Frankfurter-Black duel over due process at the expense of more central equal-protection issues. Justice Rehnquist for one might be surprised to hear Frankfurter called "the last Federalist" (p. 366). Wanting most is interpretative sweep, so impressive in the author's study of Story, linking Black's egalitarian philosophy to the profound social changes in which he played a leading part. Concentrating on personality and symbols rather than substance, the finale closes without a summation of the man and his times, leaving readers to draw their own conclusions about the stated theme.

On balance, this is an appealing synthesis of scattered materials, which excels in describing the personality and record of one of the Republic's great men. It satisfies, without foreclosing, fascination with Hugo Black and his role in the judicial revolution of recent times.

J. WOODFORD HOWARD, JR.
Johns Hopkins University

FRED L. BLOCK. *The Origins of International Economic Disorder: A Study of United States International Mone-*

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tary Policy from World War II to the Present. Berkeley and Los Angeles: University of California Press. 1977. Pp. xii, 282. $14.00.

Several insightful evaluations of the Bretton Woods system and its collapse in 1971 have appeared recently. While former Federal Reserve officials Charles Coombs and Robert Solomon offer the perspectives of experienced participants, Fred Block, a sociologist, presents a revisionist interpretation, in the tradition of William A. Williams and Gabriel Kolko, with a high level of technical sophistication.

Briefly, Block employs international monetary history to illuminate relations between the U.S. and Western Europe since World War II. While America utilized its preponderant position at Bretton Woods in 1944 to push an open world conception, based on the conviction that a free market should determine the flow of economic resources across national boundaries, Europeans leaned in a different direction. Britain's Lord Keynes favored a global system safe for "national capitalism," involving state intervention to maintain full employment and regulate foreign trade. Because the latter jeopardized the interests of American capitalism, Block claims it became a principal objective of U.S. foreign policy to block the emergence of national capitalist experiments. Indeed, Block asserts that "American policymakers were more concerned about national capitalism in Western Europe than they were with a possible invasion by the Red Army or successful socialist revolution" (p. 10).

Why did the Bretton Woods regime collapse in the early 1970s? According to Block, this order rested on a fundamental contradiction. It "worked only when American political and economic dominance in the capitalist world was absolute" (p. 163). Gradually reconstruction of Japan and Western Europe as major economic powers, together with domestic troubles, brought a redistribution of global power. Faced with mounting payments deficits and extensive global political interests, the U.S. finally broke the rules it had designed in order to gain greater freedom of action and escape the heavy burden of maintaining a fixed-exchange rate system based on dollar convertibility into gold.

This study is imaginative and provocative, but as a contribution to history it has several limitations. First, the author opted not to consult any archival materials, "because the time-consuming nature of such work would have forced me to abandon the project of analyzing both the rise and fall of the post-World War II monetary order" (p. x).

Second, convinced as Block is that an open world economy necessarily conflicts with the welfare of the world's population and that democratic socialism offers the only escape, the author embraces too easily the option of national self-sufficiency. With the world's skewed distribution of mineral and food resources, however, it is unrealistic to think that most small nations could reduce significantly their dependence on international trade without sacrificing domestic living standards. Nor does Block seem to grasp how the market mechanism benefits consumers, as well as producers, through the spread of competition and technology. Still, it is possible to praise the author's skillful presentation of his case while questioning the book's interpretations and its underlying value judgments.

ALFRED E. ECKES, JR.
Ohio State University

Executive Sessions of the Senate Foreign Relations Committee. Volume 1, *Eightieth Congress, First and Second Sessions, 1947–1948.* (Historical Series.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. v, 470. $4.05.

Executive Sessions of the Senate Foreign Relations Committee. Volume 2, *Eighty-First Congress, First and Second Sessions, 1949–1950.* (Historical Series.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. vi, 840. $6.65.

Executive Sessions of the Senate Foreign Relations Committee. Volume 3, *Eighty-Second Congress, First Session, 1951,* in two parts. (Historical Series.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. iv, 637; iii, 700. $5.25; $5.65.

Executive Sessions of the Senate Foreign Relations Committee. Volume 4, *Eighty-Second Congress, Second Session, 1952.* (Historical Series.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. vi, 781. $6.25.

These volumes cover the crucial period from 1947 to 1952. During these years the United States moved from responding to specific problems (the crisis in Greece and European economic dislocation, for instance) into the position of trying to underwrite the entire world against the perceived threat of global Communism. One can also see the growing tendency to define issues in military terms. The Berlin blockade, Russia's detonation of an A-bomb, the "loss" of China, and the Korean War combined to produce a siege mentality which is evident on practically every page of the later volumes. Indeed, it becomes commonplace to find senators and officials using the words "Kremlin" and "Moscow" in connection with topics ranging from food shortages in India to the Palestine refu-

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gee question. The Cold War, in short, had become a filter tinting every issue red.

By no means are all the executive sessions of the committee presented. Hearings on important subjects such as the Truman Doctrine, the Marshall Plan, and the formation of the North Atlantic Treaty Organization have been released as separate volumes. And matters the compilers deemed of lesser importance have been omitted (these are listed as appendixes and are available to scholars at the National Archives). Even so, the topics covered are too numerous to cite more than a sampling. They include the peace treaties with Italy, Romania, and Bulgaria; the Munitions Control Act of 1948; the Foreign Economic Assistance Act of 1950; the debate over sending United States ground forces to Europe; the Japanese Peace Treaty; and the Mutual Security Acts of 1951 and 1952. Sprinkled throughout are those sordid inquiries as to the "soundness" of men such as John Carter Vincent, Chester Bowles, and Philip C. Jessup.

There are highs and lows. Some fairly sophisticated debates over presidential powers, for example, are more than balanced by discussions of the most banal sort. Consider Senator Henry Cabot Lodge, Jr.'s penetrating query about a student exchange program: "Do you choose really representative boys or just sort of book worms and nuts?" (1: 88). Of particular interest to this reviewer is George F. Kennan's testimony prior to his confirmation as ambassador to the Soviet Union (4: 189–219). Those who believe Kennan's more recent comments about what he regarded as the misapplication of the containment policy are the results of almost thirty years hindsight should read these passages. Asked about his famous "Mr. X" article in *Foreign Affairs*, Kennan replied that he regretted he had not explained at greater length what he defined as containment. "I had not meant it as a purely military thing," he said, "I meant it primarily as a political thing. . . ." (p. 199), and elaborated as to where and under what circumstances he thought it should be applied.

These volumes are indispensible to scholars of recent American foreign policy. Others can profit from selective readings on important issues.

ROBERT JAMES MADDOX
Pennsylvania State University

J. MANUEL ESPINOSA. *Inter-American Beginnings of U.S. Cultural Diplomacy, 1936–1948.* (Historical Studies, number 2.) Washington, D.C.: Bureau of Educational and Cultural Affairs, U.S. Department of State. 1976. Pp. xvi, 365. $6.20.

By 1936 Franklin Roosevelt's Good Neighbor program faced the necessity of blunting the Axis' political, social, and economic drive into Latin America. One answer seemed to be in the promotion of close cultural ties by means of an exchange of teachers, students, and others with special hemispheric interests. The program was initiated by the Buenos Aires Convention for the Promotion of Inter-American Cultural Relations. Proclaimed in 1937, it was the first such agreement ever made by the United States. Like the entire Good Neighbor program, the cultural campaign contemplated much broader application, but was begun in Latin America because of the immediate fear of Fascism in the neighborhood.

J. Manuel Espinosa, for more than thirty years a major officer of the State Department's Bureau of Educational and Cultural Affairs, has reported the history of cultural relations in the Americas from the modest Pan American movement of the early nineteenth century to 1948. Relying heavily upon State Department sources, Espinosa also tapped his own memory and those of most of the early participants, excluding, unfortunately, the architects—Cordell Hull, Sumner Welles, and Laurence Duggan. As an inside job the book has many advantages, but can get a bit folksy. We know the first names of Department messengers; we learn where Latin American professors ate in the District of Columbia; and we discover that ladies' clubs in central Ohio began the study of Spanish in 1942. We probably have too many long quotations. In short, there may be more book here than the subject requires, for it does not go much beyond a study of the exchange of persons, north and south. (As a one-time recipient of one of these grants, I did enjoy learning how a student happened to get that opportunity.) More than 3,000 persons received similar aid, leading to one possible explanation for the Good Neighbor's consistently high reception in the academic world.

For some reason the strained relations between the Cultural Division and Nelson Rockefeller's Office of the Coordinator gets skimpy treatment. The author must have sources, if not recollections, that would clarify once and for all why Roosevelt permitted Rockefeller's duplication of the cultural campaign with far easier budgetary restrictions.

Espinosa's most careful analysis is made of the congenital fear Americans seem to have had of engaging in propaganda; almost every responsible official expressed his hope that a United States program would be clearly cultural and not "political" in the fashion allegedly pursued by the Axis.

The degree to which the exchange program succeeded is hard to measure, of course, but the thousands of Anglo and Latin American participants obviously added to an understanding of one another's regions that would not otherwise have been possible, and its continuation in a wide variety of

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forms ever since certainly attests to the need.

The book has footnotes and an index, but lacks a bibliography.

THOMAS L. KARNES
Arizona State University

ANNETTE BAKER FOX. *The Politics of Attraction: Four Middle Powers and the United States.* (Institute of War and Peace Studies.) New York: Columbia University Press. 1977. Pp. viii, 371. $15.00.

This book is a study of the interaction since 1945 between the United States and four representative, friendly "middle powers"—Mexico, Brazil, Canada, and Australia—which Fox defines as countries "in the aggregate less influential" but at times equally or more influential than the so-called "second-tier" powers (e.g., Britain and France). Deriving her material largely from secondary sources, she examines the four sets of bilateral relationships in the areas of security, foreign trade, direct investment, global problems, and "neighborhood" issues (restricted to Canada and Mexico).

Those familiar with the postwar international context will have little difficulty accepting Fox's findings. Her examination revealed, for example, that crisis situations induced security cooperation. On economic issues, however, cooperation increased under conditions of prosperity, especially when the governments involved were able to transcend domestic political claims, and the disparity in economic strength between the United States and the other country was clearly visible. She also found that the middle powers preferred reciprocal arrangements to joint enterprises, and that they usually specified the extent of their cooperation via carefully negotiated agreements, in order to confine spillover, and to promote at least the appearance of maintaining an independent foreign policy. In general, proximity exerted a negative influence on foreign policy coordination, but a positive impact on neighborhood issues, such as river development and environmental quality. Economic problems were normally handled administratively and only infrequently became politicized. Security and global issues, on the other hand, usually engaged high political levels. In short, Fox concludes, initiatives and responses varied with the nature of the issue, and "mutuality of advantage was not ruled out by asymmetry in power rank" (p. 290).

I have only one major reservation about this book. The author offers a curiously narrow conception of interaction between nations. Although she alludes to certain factors affecting the process of interaction, her approach appears to suggest that initiatives and responses are in some cases self-generating and in others reducible to a matter of personalities. In reality, complex international decision-making systems are responsible for actions taken by one state toward another, irrespective of whether the two states generally maintain friendly relations. In the United States, the decision-making system involves, *inter alia*, extensive interagency bargaining. This bargaining process, for instance, is no less a determinant of action than geographical proximity or the existence of channels of communication. Some analysis or at least more explicit appreciation of this aspect of the relations between the United States and the four selected middle powers would, I think, have enhanced the overall value of the study.

This reservation aside, *The Politics of Attraction* is a useful book. The author's discussion is wide-ranging and often insightful, particularly with respect to the role of multinational corporations, and the impact of increasing economic interdependence on a middle power's ability to maintain an independent posture in foreign policy. This study will fill a real need among those seeking an informed account of United States policy toward these four countries, and also those in search of enlightenment about how friendly states of unequal power manage their relations.

N. STEPHEN KANE
U.S. Department of State, Office of the Historian

THOMAS A. BRYSON. *American Diplomatic Relations with the Middle East, 1784–1975: A Survey.* Metuchen, N.J.: Scarecrow Press. 1977. Pp. viii, 431. $17.50.

Thomas Bryson's study is in the form of an analytical narrative depicting the evolution of U.S. diplomatic relations with the Middle East from the founding of the Republic to the present. His treatment of the subject clearly reveals his preference for the "realist" approach to the study of U.S. foreign affairs and focuses upon the extent to which the United States has pursued a policy consistent with the "national interest." As regards the radical and/or revisionist viewpoints, however, Bryson displays little sympathy, arguing as he does on several occasions that official U.S. policy actually ran counter to the interests and objectives of U.S. oil companies in the Middle East.

Bryson tends to define the U.S. "national interest" in terms of the rising domestic demand for oil and the need to insure adequate access to the impressive oil reserves of the Arab world. In this regard he is highly critical of U.S. policy during the Truman and Eisenhower administrations. He

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criticizes Truman for having "a narrow view" (p. 170) of the Middle East and depicts his decision to support the creation of Israel as "A Great Aberration" (p. 161). The policy of the Eisenhower administration also fares poorly under Bryson's scrutiny. He labels the Baghdad Pact and the Eisenhower Doctrine of the 1950s as "dysfunctional" (pp. 187, 206) insofar as they deviated from previous attempts to achieve "a rational accommodation" (p. 94) with the realities of Arab nationalism. As a consequence, they tended to foster the growth of Soviet influence throughout the Middle East, the exact result which Eisenhower's policy had sought to prevent.

By way of summation Bryson calls into question the wisdom of pursuing the "special relationship" with Israel that evolved during and after the June 1967 War; and he praises Henry Kissinger for pursuing a policy of restraint during the October 1973 War. For the long term he recommends that the U.S. should discard the "illusion of an imaginary common interest" (p. 301) with Israel and establish a policy based upon "a true community of interests" (p. 305), which he defines as assured and unlimited access to Arab oil reserves, coupled with an expanded opportunity for U.S. firms to take full advantage of the potentially large Middle Eastern market. In brief, his arguments are straightforward and compelling. Still, the reader feels obliged to pose the following questions: 1.) Is it possible to bring about a fundamental change in Washington's foreign policy given the realities of domestic politics in the United States? and 2.) Is it possible to investigate in the best tradition of the "realist" perspective the basic origins and thrust of America's policy in the Middle East without allowing for the continuing impact of certain domestic considerations?

ANTHONY R. DE LUCA
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Foreign Relations of the United States, 1948. Volume 5, *Near East, South Asia, and Africa.* Parts 1 and 2. (Department of State Publication, numbers 8802 and 8840.) Washington, D. C.: U.S. Government Printing Office. 1975, 1976. Pp. x, 532, xv; xvi, 533–1730, xxxi. $8.25; $8.25.

Under Secretary of State Robert Lovett, resenting the destructive effect of partisan politics on American foreign policy in the 1948 presidential campaign, complained vigorously against the "silly season" (p. 1528). The documents in part 2 of this *Foreign Relations* publication illustrate well that electoral politics governed Harry S. Truman's reaction to the Arab-Israeli controversy in Palestine. Consider the following random episodes: Secretary of State George C. Marshall bluntly told the president that he would vote against him if Truman recognized Israel for "domestic political considerations" (p. 975); the American delegation to the United Nations almost resigned en masse when the president abruptly recognized Israel on May 14; when the president appointed James G. McDonald as U.S. special representative to Israel, a stunned Lovett asked who McDonald was, and presidential assistant Clark Clifford firmly, even haughtily, responded that there was "no room for argument" (p. 1131); Truman instructed the State Department to cease commenting on Palestine questions "until after [the] elections" (p. 1490). All in all, the 1,200 pages of part 2 demonstrate that Democratic Party aspirations in 1948 (Clark Clifford's disingenuous 1976 American Historical Association paper notwithstanding) dictated a confused American policy toward the Middle East.

Part 2 suggests other themes as well. The United Nations, which passed resolution after resolution and instituted numerous truces, was shouldered aside, never able to provide mechanisms to reduce the violence. Hardworking Ambassador to the United Nations Warren Austin diligently tried to give teeth to UN decisions and mediating efforts, but found that the White House frequently undermined him, leaving his credibility diminished. The White House and the State Department worked at cross-purposes. Clifford denounced the "shilly-shallying appeasement of the Arabs" (p. 695) and favored early recognition, whereas State, sensitive to Arab opinion and alarmed by Israeli "aggressive tendencies," as Marshall put it (p. 1364), wanted to try a trusteeship and the Bernadotte plan. Throughout, both Truman and State criticized "British bullheadedness," to quote the president (p. 593), and the Anglo-American coalition was strained. The Policy Planning Staff under George F. Kennan's direction prepared reports alleging a Soviet menace, but these exaggerated statements seemed to have drawn minimal attention in the absence of any important evidence of Soviet meddling in Palestine. Secretary Marshall does not appear to have been a central figure in the creation of foreign policy, leaving that to Lovett and the various desks of the State Department. The well-documented plight of the several hundred thousand Arab refugees concerned Washington, but that problem did not determine policy. The State Department, which sought to delay recognition of Israel to give mediation a chance, to put pressure on the Israelis to halt truce violations, to permit Arab refugees to return to their lands, and to give American support to the Bernadotte plan, met rebuff from the Truman-Clifford White House. Moreover, when Israel broke the UN truce in December, the United States abstained from voting on a UN resolution of protest and then

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promoted UN membership for Israel.

But as these vast materials make evident, the participants and events themselves, not the United States, controlled the outcome in the Middle East. Arabs and Jews were determined to fight, and it seemed there was little outsiders could do. One senses in this historical record the helplessness of outsiders and the uncertainty and difficulty of settling upon a policy. Clifford took the practical and politically rewarding course: nationhood for Israel was "inevitable," he said, so why should the United States not be first in line to recognize the new state (p. 909)? In short, the United States accepted what it perhaps could not and did not wish to change. Jewish voters would favor the beleaguered president against Thomas Dewey, and the United States would gain a Cold War friend, for Israeli leaders time and time again reminded American officials that they were staunchly anti-Communist. As for the Arabs, whenever Clifford or Truman thought about them, it was assumed that they were basically powerless and could be persuaded to sustain their ties with Washington.

Part 1 includes material on American petroleum policy; United States fears of losing Arab oil concessions; the importance of Middle Eastern oil to the European Recovery Program; growing United States influence in Iran through military missions and foreign aid, accompanied by loud Soviet protests; American encouragement of British influence in Iraq; assistance to Saudi Arabia, in part, to continue American use of the Dhahran air base; the Kashmir dispute between India and Pakistan and the United States' expression of distaste for India's decision to take a neutral stand in the Cold War; and American commentary on "repression" in South Africa, but the primacy in United States policy toward that country of "strategic considerations" and a "good market" (p. 531). The two parts, in general, illustrate that the United States was struggling to establish the areas of primary focus ("locales") and the availability of power in its various forms ("methods") to implement the containment doctrine on a global scale (p. 186). The Truman Doctrine and the program of aid to Greece and Turkey in 1947 kept coming up as a precedent for action elsewhere. The global implications of containment were evident to policymakers, as were the difficult choices of judging "where, how, and to what extent [the U.S.] can extend assistance" (p. 86).

Part 2 reflects some welcome changes in the presentation of the *Foreign Relations* series, and the Historical Office deserves our applause for issuing an important volume on such a controversial subject of current international debate. This volume should inform that debate. For scholars, a long introduction explains the editors' procedures and the kinds of sources they used. The footnotes are much fuller and will help researchers trace other relevant sources. The editors made a considerable effort to include non-State Department materials: Clifford memoranda, Truman letters, UN documents, and a handful of Central Intelligence Agency and National Security Council reports. Because policy was often made outside the Department of State, these documents are crucial—and they will become more so in future volumes.

But there is room for further improvement. The records of the National Security Council, especially minutes of meetings located at the Truman Library, receive almost no space here, presumably because the Historical Office could not persuade the NSC to declassify them. Several scholars are now applying for declassification of the NSC minutes, but we should know from the introduction whether and to what extent the editors tried and failed to declassify such essential materials. The editors should tell the users of these *Foreign Relations* volumes not only which documents were printed from non-State-Department sources, but also which documents were denied to them and remain classified. The "Catch-22" answer from the Historical Office to this suggestion is that the statement of information on classified materials itself is classified. That may be so, but as nongovernmental scholars we have to point out that our access to the historical record is thereby denied and that we must demand a full reckoning. Part 2 also includes a list of "published works" which invites this reader's skeptical questioning. Does it have a place? The list will become quickly dated, and the footnotes themselves indicate which published works the editors used. The list might be taken by the unsuspecting as balanced. Yet Margaret Truman's biography of her father is listed, while John Morton Blum's *The Price of Vision*, on Henry Wallace, is not. There are several listed works by Israelis but none by Arabs.

The *Foreign Relations* series as a whole is an absolutely vital tool for scholars, and it behooves us to press for improvements, as well as to inform elected officials in Washington, especially the secretary of state, that we so value the contribution of the Historical Office that it should be granted a larger staff and more of our tax dollars. Above all else, it is imperative that other offices of the State Department and other agencies of government, such as the National Security Council and the Central Intelligence Agency, become more cooperative in the opening of historical records to the scrutiny of the American people, in whose name, after all, foreign policy was and is made.

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RAY S. CLINE. *Secrets, Spies, and Scholars: Blueprint of the Essential CIA*. Washington, D.C.: Acropolis Books. 1976. Pp. xii, 294. $10.00.

During the recent orgy of revelation, incrimination, and investigation of the C.I.A., it was all but forgotten that the agency was not merely "an invisible empire," in which a group of willful men, with boundless funds and little conscience, engaged in illegal if not criminal enterprises altogether contrary to national traditions and interests. Even before the predecessor of the C.I.A., "Wild Bill" Donovan's wartime OSS, and even before the OSS, there existed as part of the new foreign intelligence organization the Office of the Coordinator of Information, the objective of which was to centralize all the varied foreign intelligence coming into the government, to subject it to systematic analysis by highly qualified personnel, and to distill from it, so far as possible, a single, agreed estimate of problems touching the national security. One of Donovan's principal and most fruitful ideas was to tap the universities for specialized personnel with intimate acquaintance with the languages, history, and social conditions of foreign parts. What became the R and A Branch of the OSS when the original agency came under the supervision of the Joint Chiefs antedated all the paramilitary operations which the OSS was called upon to undertake once the United States was an active participant in the war. It was natural that, when the great conflict came to an end, the heroic and mostly successful feats of the operational branches should have completely overshadowed the unspectacular research and analysis of the impressive concentration of scholars who prepared hundreds of reports and by 1945 had made an enviable name for the R and A in a capital which had by no means welcomed it.

For some years after the war the entire foreign intelligence situation remained uncertain and confused. The OSS was unceremoniously dissolved in October 1945, only to be revived as the C.I.A. in 1947. In the tensions of the Cold War it was realized that secret operations were still necessary, and they presently expanded to the point where they became synonymous with the entire agency. Little was heard and even less was known of the drastic reform of the intelligence analysis or coordination carried through in 1950 by General W. Bedell Smith as Director of Central Intelligence. Through the new Office of National Estimates Smith imposed on the other intelligence agencies cooperation in the drafting and ultimate establishment of national estimates which before long became recognized as among the few documents crucial to the formulation of foreign policy. Over the past twenty-five years this work has gone on without publicity or fanfare, while the secret operations of the C.I.A. were exposed first to criticism and then violent attack.

This rather lengthy preamble is essential to an understanding of Ray Cline's book, which may justly be described as the first authoritative statement of the proposition that the coordination and evaluation of foreign intelligence is the very crux of the foreign intelligence problem and that, as an ongoing business, it is more important than the so-called "dirty tricks" which may from time to time be required in the national interest. Very few persons have Cline's qualifications for writing such a book. As a young man and a junior fellow at Harvard he came to the research branch of the OSS early in its history. Having decided to remain in government service after 1945, he rose rapidly over the ensuing twenty years from one rank to another in the C.I.A., serving in the operational divisions as in the research activity and devoting years in important foreign stations, where he was able to oversee all the varied activities of the agency. Eventually he became Deputy Director of the C.I.A. before transferring to the Department of State, where he ended his career as chief of the intelligence organization. Out of his long, varied, and intimate experience he is able to speak with great authority and, as a trained historian, to review the innumerable, knotty problems of intelligence work. He has revealed the other side of a much-maligned agency and demonstrated the continuing contribution made by it in matters touching the national security.

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LEWIS MCCARROLL PURIFOY. *Harry Truman's China Policy: McCarthyism and the Diplomacy of Hysteria, 1947–1951*. New York: New Viewpoints. 1976. Pp. xv, 316. Cloth $15.00, paper $6.95.

The extraordinary fervor displayed by the American government, until recently, in "containing" China—including bloody wars in Korea, Vietnam, Laos, and Cambodia—contrasts sharply with the Truman administration's earlier decision, publicly asserted as late as January 1950, to write off the Asian mainland as beyond America's defensive perimeter. This momentous policy shift is the subject of Lewis M. Purifoy's pungent study, which concludes: "It was the frenzied McCarthyite attacks upon the government for its alleged 'softness on Communism'—and only this—that brought about the reversal of America's policy of disengagement from the Asian continent" (p. xiv). Fending off the real or anticipated attacks of the American Right, "the Truman Administration

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and its four successors ran madly in pursuit of Communists around the periphery of China," always convinced, in the words of Daniel Ellsberg, that "this is not the year to lose Asia to the Communists" (pp. 305–6).

Purifoy presents his argument lucidly and forcefully. Although one might wish that the author had tempered his tone or foregone the frequent use of such terms as "commie-bogie," "Asialationists," "containmentism," and "conspiracists," he maintains superb control of his evidence, with which he assembles a powerful case. Unfortunately, the evidence is drawn entirely from printed sources, which, while manifold and ably handled, do not constitute the archival research necessary for a definitive study. Even so, Purifoy's book is exceptionally persuasive, and merits careful appraisal in monographs on U.S. policy toward China, Korea, and Vietnam.

At the base of Purifoy's thesis lies the contention that the American people demanded a more vigorous anti-Communist crusade. Yet the author cites no polls to buttress this point, which contradicts recent studies by Michael Rogin, Robert Griffith, and Athan Theoharis. Indeed, it appears that U.S. wars in Asia were a political liability rather than a political asset to U.S. policymakers. Moreover, one wonders if the Truman administration's pre-1950 policy toward China was quite as reasonable and disinterested as Purifoy would have us believe. The State Department's 1949 White Paper recounted the longstanding U.S. efforts to hold the line for Chiang Kai-shek, with Dean Acheson concluding that "the unfortunate but inescapable fact is that the ominous result of the civil war in China was beyond the control of the United States." But, as Purifoy himself asks, what business had the United States seeking to control China's civil war? If, as he contends, U.S. Asia policy after 1950 represented "madness," were not U.S. policymakers—with the exception of a few China specialists—already a bit nutty?

Nevertheless, the author makes an impressive case for the impact of the domestic Red Scare upon American diplomacy. Recalling "the attack of the primitives . . . on the China policy of the Truman Administration," Acheson maintained that "whatever effect it had on our successors, it had little on us." In the light of Purifoy's study, Acheson's assertion must be called into question.

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Foreign Relations of the United States, 1949. Volume 7 *The Far East and Australasia*, part 1. (Department of State Publication, number 8797.) Washington, D.C.: U.S. Government Printing Office. 1975. Pp. x, 600, xiii. $8.75.

Foreign Relations of the United States, 1950. Volume 6, *East Asia and the Pacific.* (Department of State Publication, number 8858.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. x, 1581. $16.00.

The gigantic number of documents in the two volumes covered by this review will be carefully examined by historians who will discover, although there are a few minor discrepancies, that generally the data support the major current historical interpretations of the period. All of volume 7 (1949) and over one third of volume 6 (1950) is concerned with French Indochina and Indonesia, where wars of national liberation were in process. The United States was extremely critical of French policy in Indochina. In 1949, Washington was constantly informing Paris that the limited reforms granted to the Vietnamese were inadequate to satisfy the nationalist desires of the people. The State Department realized that Ho Chi-minh was a Communist but believed his major appeal was nationalism. The attitude of the United States hardened in late 1949 and early 1950, particularly when the Soviet Union and Communist China recognized Ho Chi-minh's Democratic Republic of Vietnam. In reaction, Washington decided to recognize the newly created state of Vietnam under former emperor Bao Dai, even though it was obvious that the French maintained control under a facade of independence. Even after the eruption of the Korean War, the State Department continued to advise the French to grant additional reforms but with less pressure than previously. Although the United States decided to grant military and economic aid to the newly formed state of Vietnam, the State Department rejected any possibility of commitment of American troops in the area even "in the event of overt attack by organized Chinese Communist forces" (7: 951).

In 1949 and 1950, United States policy on Indonesia was consistently anti-Dutch and pro-Republican. In December 1948, over the protests of the United States and the United Nations, the Dutch instituted a second "Police Action" against the Indonesian Republic. Washington refused to cancel European Recovery Aid to Holland, but did indicate it would support a resolution in the United Nations branding the Dutch aggressors. Under pressure from the United States and the United Nations, the Dutch government reluctantly convened a "Round Table Conference" at the Hague in August 1949 which basically granted Indonesia independence in December. In 1950, the United States established diplomatic relations

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with the new Republic of the United States of Indonesia and began to grant military and economic assistance. The problem on the horizon was the question of Western New Guinea, which was retained by the Dutch and was demanded by the Indonesians. Washington supported the Dutch primarily because Australia considered New Guinea essential to its security and did not desire Indonesian control.

Much of volume 6 (1950) deals with Taiwan and Japan. In early 1950 American policy toward Taiwan was one of noninvolvement. Secretary of State Dean Acheson stated "it was not in the interest of the American people to hazard a war over Formosa" (p. 260), and proclaimed that the strategic value of Taiwan to the United States was extremely limited. The State Department assumed that Communist China would launch a successful military campaign against Taiwan. As late as May the chargé in Taipei recommended that all Americans be "strongly advised" (p. 340) to leave Taiwan while he also developed plans for the evacuation of American diplomatic personnel. The Defense Department, particularly General Douglas MacArthur, was simultaneously attempting to persuade President Truman to increase support for Chiang Kai-shek. Initially, President Truman sided with the State Department, but when the Korean War erupted in June 1950 the Defense Department's position triumphed. On June 27, President Truman ordered the Seventh Fleet to prevent any attack on Formosa by Communist China and subsequently reinstated military and economic aid to Chiang. Truman did refuse to implement Defense Department recommendations to allow Chiang to engage in offensive operations and ordered the Seventh Fleet to prevent any air or sea operations against the China mainland. Truman's decision irritated General MacArthur who continued to argue for a greater commitment to Chiang, occasionally getting himself in trouble with Washington, although Truman was reluctant to censure the general publicly. Many officials of the State Department were not pleased with Truman's decision to protect Taiwan. For example, George Kennan, counselor to the secretary of state, was critical of the Nationalist Chinese leadership and regarded the Taiwan regime as "wholly unreliable" (p. 381). In September 1950 Robert C. Strong, formerly chargé in Taipei and later in the Office of Chinese Affairs in Washington, critically described the Nationalist Regime on Taiwan as dictatorial, engaging in a reign of terror, and its armed forces as incompetent (pp. 485–87).

The question of a peace treaty dominates the 300 pages of documents on Japan. Detailed recommendations and many, many draft treaties and analyses of these drafts by men such as General MacArthur, William Sebald, John Foster Dulles, and George Kennan, as well as by the Joint Chiefs of Staff, Britain, France, and even India, are included. The Korean War stimulated additional speed, and a decision was made to proceed with the peace treaty as quickly as possible over the objections of the Soviet Union and Communist China. The treaty was not actually signed until 1951.

In volume 6 (1950) there are a few pages of documents on the other Asian areas: 39 pages on Australia; 130 pages on the Philippines; 10 pages on Thailand. Although coverage of Burma is equally brief, only 26 pages, it illustrates one of the basic problems with the *Foreign Relations* series. Periodically, State Department recommendations as revealed through the documents are at variance with the actual policy pursued by the American government. In 1950, five thousand Nationalist Chinese (KMT) soldiers fled from Communist China into Burma. The State Department recommended immediate evacuation to Taiwan, but over the next twelve years the Central Intelligence Agency blocked the evacuation and provided the KMT troops in Burma with military and economic aid.

In the *Foreign Relations* series little searching is necessary to discover desired information. Each country is usually introduced by a general policy statement which enables the researcher to quickly determine the major diplomatic trends. Invariably the volumes have excellent indexes, and the texts have extensive explanatory and cross-reference footnotes.

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Foreign Relations of the United States, 1950. Volume 7, *Korea.* (Department of State Publication, number 8859.) Washington, D.C.: U.S. Government Printing Office. 1976. Pp. xiii, 1675. $17.00.

Volume 7, of the *Foreign Relations of the United States,* embracing Korea in 1950, constitutes a dramatic peak in the essential but often tedious *Foreign Relations* series. Edited with customarily high professionalism by the late S. Everett Gleason and others of the Department of State, with obvious and invaluable assistance by the historians of the Department of Defense, this volume now appears six years later than the American Historical Association's current goal for the publication of U.S. Government documents, i.e., twenty years after the event.

This volume opens with the optimistic reports of the spring of 1950, both from the U.S. Embassy in Seoul and from the still embryonic Central Intelligence Agency in Washington, concerning the

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relative strengths of the South Korean and the North Korean forces. Following the not entirely unexpected attack by the North Koreans on June 24, 1950, official American opinion changed abruptly. Nonetheless, the Department of Defense and the Joint Chiefs of Staff still reflected little enthusiasm for committing U.S. ground troops, although, significantly, General Hoyt Vandenberg, Chief of Staff of the U.S.A.F., had already begun to discuss the employment of atom bombs against Soviet jets based at Shanghai.

By June 28 Secretary of State Dean Acheson wrote that while the American decision "to commit United States air and naval forces to provide cover and support for South Korean troops does not *in itself* constitute a decision to engage in a major war with the Soveit Union if Soviet forces intervene in Korea." Nevertheless this decision "was taken in the full realization of a risk of war with the Soviet Union" (p. 217). This new policy, reiterated on the next day by the J.C.S. in a directive to General Douglas MacArthur in Tokyo, already permitted some limited use of U.S. Army service and combat troops in the area of the vital South Korean port of Pusan. By June 30, however, following a brief personal inspection in Korea, MacArthur was forced to report that the North Koreans could no longer be held without U.S. infantry.

Thereafter, the famous struggle of the Truman administration and of the Joint Chiefs of Staff to limit the war in Korea against the ever growing imperatives of Douglas MacArthur and of enemy advances were paralleled within the State Department by a sotto voce dispute between the more hawkish Office of Northeast Asian Affairs and the more dovish Policy Planning Staff regarding how to unify all of Korea under the authority of the United Nations. Endless attempts to mediate the war, often under the somewhat dubious auspices of the Indian Government, likewise resemble the comparable problems of the subsequent limited war in Vietnam. The unpopularity and brutality of Syngman Rhee's government of the Republic of Korea is also candidly described.

With MacArthur's counteroffensive at Inchon, fears of Chinese Communist entry became manifest, usually under the questionable impression that Peiping (Peking) was little more than a cat's paw for the Soviet Union. Resolutely, the National Security Council then opposed expansion to a larger war with China, although following the latter's intervention in Korea in November, bitterness against the "Chi Commies" (p. 1532)—in the vulgar parlance of the time—welled up even among State Department doves.

Throughout the many government agencies cited here a striking inability to grasp Chinese Communist thought obviously led to MacArthur's fiasco before the Yalu. The general himself was only the most spectacular participant in this failure, while the much-blamed C.I.A., if it did not do well in predicting the Chinese intervention, at least did better than most others in the U.S. government. Certainly, plenty of evidence regarding support for MacArthur's eventual program of bombing, blockade, and employment of Chinese Nationalist forces against Peiping emerges in these pages, especially among the Joint Chiefs, to whom MacArthur would turn in vain following his recall in the next spring.

In the climactic talks with Prime Minister Attlee of Great Britain in December 1950, precipitated by well-justified British anxiety concerning the rapid expansion of the Korean War in several ominous new directions, ranging from nuclear weapons to Taiwan and Hong Kong, Dean Acheson hung onto his faltering control of the war. Giving in neither to the desperate and humiliated MacArthur on the one hand nor to the fearful and reluctant British on the other, during these talks Acheson laid the basis for the eventual mediated peace in Korea in 1953. In retrospect, at least after the total failure in Vietnam, Acheson's and, to a lesser extent, Marshall's and Bradley's thankless achievement grows in magnitude. But, then, the stature of the men inherited by Truman from Roosevelt was incomparably greater than that of those bequeathed by John Kennedy to Lyndon Johnson.

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DAVID DETZER. *Thunder of the Captains: The Short Summer in 1950.* New York: Thomas Y. Crowell Company. 1977. Pp. xi, 243. $10.00.

David Detzer undertook an ambitious effort when he conceived *Thunder of the Captains.* His primary goal was to describe and analyze the first two weeks of the Korean War from various perspectives. A second task was to demonstrate the effect of the major policy decisions on the lives of the people affected by those decisions. The results are impressive and may serve as an example of the integration of diplomatic, political, and social history.

Detzer has marshaled evidence from a number of different sources—manuscripts, interviews, and the published record—to indicate how and why the United States became involved in the Korean War. Most illuminating on this point is the fact that by 1949 the United States Army had declared Korea militarily and strategically unimportant for

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the protection of the United States. Why then did the country go to war for South Korea in 1950? Detzer astutely demonstrates that certain factors, diplomatic and political, came into play. Among the most salient were the actual North Korean attack, the Russian explosion of an atomic bomb, the involvement of the State Department in the decision to intervene, the Asian situation, and the attitudes of Harry Truman. Therefore, a combination of events, Cold War beliefs, and the leadership of Harry Truman led the country to fight in Korea.

Detzer's conclusions are reasonable, persuasive, and indicative of his skillful use of sources. The work is also praiseworthy because of the attempt to humanize this two-week period. His descriptions of General Douglas MacArthur are memorable. The accounts of the confusion and horror of the fighting are powerful and dramatic and fully capture the ugliness of battle. At the same time his picture of the United States in 1950 helps the reader better understand the social fabric of America at the midpoint of the twentieth century. In blending the various strands of his account together Detzer has produced a welcome addition to the literature of the period in which he clarifies a great many issues that are part of an unclear and muddled situation.

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STUART I. ROCHESTER. *Takeoff at Mid-Century: Federal Civil Aviation Policy in the Eisenhower Years, 1953–1961*. Washington, D.C.: U.S. Department of Transportation. 1976. Pp. viii, 352. $6.00.

The first of four volumes on the history of the Federal Aviation Administration (FAA) and its predecessors, Stuart Rochester's study fulfills its purpose in the narrowest terms of history. Problems arising from increased air travel and the introduction of commercial jet aircraft are described from the bureaucratic perspective of the Civil Aeronautics Administration (CAA), a third tier divison in the Department of Commerce that became the FAA in 1959.

Rochester composes a well-written guide through the thicket of divergent factors influencing aviation policy. He clarifies a maze of technical issues including positive traffic control, airport and airway location, airspace allocation, flight standards, safety regulations, accident investigations, and pilot examination and certification. These perpetual issues appear incapable of permanent solution while technology progresses.

The primary emphasis is on conflicts between personalities and intergovernmental groups seeking to control America's aviation policies. The CAA sought authority to promote and regulate aviation's rapid growth amidst intra-office strife while contending with outside forces such as budget-pinching Secretary of Commerce Sinclair Weeks, strong-willed military aviation leaders, favor-seeking commercial and private aviation groups, and pork-barrel oriented congressmen.

The author's focus on bureaucratic struggles discloses muddled government activity. Policies lag behind events and decisions by the administrations or Congress require the stimulus of periodic aircraft collisions. One concludes that American aviation advanced despite federal policy-makers' ineptitude.

Civil aviation policy changes demonstrate the deficiencies of President Eisenhower's "dynamic conservatism." Between 1953 and 1956, slashes in CAA personnel (1,400 jobs lost in two years) and budget (from $187 million to $132 million) exacerbated aviation problems precisely when jet planes were introduced. Conservative attempts to give local communities greater responsibilities were ineffectual because a national air program was required for satisfactory traffic control, aviation regulations, airspace usage, and related issues. Local interference complicated or hindered the national effort.

By 1956, Eisenhower and Weeks reversed prior commitments. They increased the CAA budget to $573.5 million and added over five thousand employees by 1960. Moreover, the CAA moved from the Commerce Department and, as the FAA, became a centralized agency with greater authority than previously.

Rochester's narrow intent becomes a handicap at this point. He describes events but does not consider why Eisenhower and Weeks changed their program. In addition, the bureaucratic focus results in a blurred picture of aviation's fantastic growth and the national effort essential to that development. Statistics of aviation growth are cited but are not attributed to backward civil aviation policy or any other effective cause.

This study concerns what CAA-FAA officials needed, wanted, and eventually received. The explanation for civil aviation's "take-off" is left for other researchers. Nevertheless, if the three remaining volumes of the FAA series achieve Rochester's commendable standards, historians will have valuable sources for broadly conceived administrative studies of the era 1926–72.

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ARNOLD H. TAYLOR. *Travail and Triumph: Black Life and Culture in the South Since the Civil War*. (Contribu-

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tions in Afro-American and African Studies, number 26.) Westport, Conn.: Greenwood Press. 1976. Pp. viii, 325. $15.95.

In his preface Arnold Taylor sets forth the aims and scope of *Travail and Triumph*: "Because the main purpose of this study is to provide students on several levels with a body of basic information that will enhance their understanding of the historical circumstances of Southern black life and the evolution and functioning of the Southern black community, the subjects chosen for discussion are fairly traditional and are treated, for the most part, in a traditional way. No attempt has been made, therefore, to impose an overall abstract and complex conceptual framework upon the issues discussed."

While this is a concise description of Taylor's method, which is narrative synthesis, it does not do justice to the range of topics he treats. Unlike most historians, Taylor pays attention to the music, tales, and religion of the folk, and while one wishes that these subjects were related more fully to the political, social, and economic data treated in separate chapters, their very inclusion is an occasion for rejoicing. The main problem with this work is neither its method nor its scope, but its brevity. Taylor's excellent account of folktales is too brief. His important picture of the black church as a source of community prestige and power is not as convincing as it could have been because it lacks sufficient detail concerning the role the church played in the lives of black people. When he does become detailed, Taylor's choices are often surprising. He tends to spend too much time giving the names of songs, books, poems, writers, and performers, and too little in supplying examples of the material and exploring its ethos. Why devote two pages to James Weldon Johnson's poetic rendition of black sermons in order to give their essence when he could have used such sermons themselves? Why devote more than a page to listing the names of black educational institutions founded after the Civil War when the space could have been used to enlarge the important discussion of what the freedmen found when they went to these schools?

Works of comprehensive synthesis are, of course, particularly open to criticisms concerning matters of priority and detail, and other readers will doubtless find their own favorite areas of concern. Of more fundamental importance is the question of whether Taylor has fulfilled his original goals. On the whole I think he has. He has given us an unpretentious, straightforward, well-written, and helpful introduction to black Southern history since the Civil War. He has handled a large volume of material clearly and cogently and along the way made a series of perceptive observations on subjects as diverse as black political power during Reconstruction, the motives underlying black migration from the South, the differences between black and white Populists, the contours of black marriage and family, and questions of color and class among black Americans. Again and again he provides illustrations of his central theme that "throughout their history, black Southerners have been more than merely passive victims of an oppressive social order. . . . they have responded variously to their social and physical environment and have frequently seized the initiative in shaping their individual lives and in developing a viable community life and a rich expressive culture."

LAWRENCE W. LEVINE
University of California, Berkeley

DANIEL J. LEAB. *From Sambo to Superspade: The Black Experience in Motion Pictures*. Boston: Houghton Mifflin. 1975. Pp. viii, 301. $15.00.

"Except for a few notable exceptions like *Sounder*, *The Learning Tree*, and *Nothing But a Man*, the film image of the black is as condescending and defamatory as it has ever been. . . . Whether as Sambo or Superspade the humanity of black people is still being denied in the movies," Daniel J. Leab argues in the conclusion of his study of the black experience in motion pictures. It is a convincing argument which he documents well by tracing the history of black images on the "silver" screen from *Rastus in Zululand* (1910) to *Shaft in Africa* (1973).

No one who is genuinely interested in Afro-American (and mainstream) cultural history can be displeased when a serious scholar undertakes research in an area as important and influential as film. Blacks were for many years—apologies to Ralph Ellison—the invisible men/women of film history and criticism. The first major study was undertaken in the late forties by Englishman Peter Noble (*The Negro in Films*, published in London). American scholars did not exactly seize the initiative, for it was 1972 before another full-length study was published, Edward Mapp's *Blacks in American Films: Today and Yesterday*, followed in 1973 by Donald Bogle's *Toms, Coons, Mulattoes, Mammies, and Bucks* and James Murray's *To Find an Image, Black Films from Uncle Tom to Super Fly*.

Leab's book is sufficiently different from the others to warrant our attention. Using the tools of the historian, he organizes his work chronologically and carefully marshals the evidence to support his thesis: *plus ça change, plus c'est la même chose*. Blacks entered films as caricatures—not even played by black actors but by whites in blackface—and have

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through the years merely exchanged one stereotype for another. With memorable exceptions, he says, the roles are shallow, demeaning, and without basis in the reality of black experience.

From Sambo to Superspade covers the major Hollywood films, as well as those films made primarily for black audiences by such pioneer black filmmakers as William Foster, Noble and George P. Johnson, and Oscar Micheaux. There are inevitable omissions (e.g., Evelyn Preer's important career as the star of the Lafayette Players) and some generalizations, to be expected in a book that attempts to encapsulate the total black experience in films in three hundred pages. Thomas Cripps' study, *Slow Fade to Black* (1977), which he has been working on for a number of years, runs to 477 pages in the first volume and only covers the period from 1900 to 1942. We do have here, however, a work of considerable substance and merit.

PHYLLIS R. KLOTMAN
Indiana University, Bloomington

MORRIS DICKSTEIN. *Gates of Eden: American Culture in the Sixties*. New York: Basic Books. 1977. Pp. xi, 300. $11.95.

This eloquent book about the "new sensibility" of the 1960s should have been subtitled "American Writers and American Culture in the Sixties." Although it takes a wide cultural approach throughout, the work focuses on literary craftsmen of the fifties and sixties (Ginsberg, Mailer, and Bellow, among others) as prophets of cultural revolt. Social critics such as Paul Goodman, Norman Brown, C. Wright Mills, and Herbert Marcuse are also evaluated as harbingers of change. Black writers (especially Richard Wright, James Baldwin, and Ralph Ellison) have their own chapter. Even Bob Dylan and the Rolling Stones are analyzed in a chapter on rock music lyricism. Dickstein's cast of intellectual giants has no surprises; all are generally acknowledged as peculiarly relevant to the 1960s. Feminist writers are the glaring omission (feminism is not even mentioned in the index). Surely Kate Millett and Erica Jong can tell us much about the sixties.

The final chapter, "Remembering the Sixties, Surviving the Seventies," traces the author's recent life, while making final generalizations about the meaning and legacy of the last twenty years. Dickstein, who enrolled at Columbia University in 1957, did graduate work at Yale, and taught at Columbia during the SDS eruptions in 1968, should have provided his biography in an introduction, for this entire book is, in part, personal memoir. Moreover, it functions throughout as a survival manual for those trying to make some sense and use of the sixties.

Dickstein sets himself apart from "many highbrow critics" who "are still unable to acknowledge . . . that the line between popular culture and high culture gave way in the sixties" (p. 186). However, *Gates of Eden* is somewhat limited by literary biases common to *New York Review of Books* critics. Thus, Dickstein's perceptive interpretations of authors and literary genres sometimes tell us more about the writer and his place than his cultural effect. We learn more about Mailer and less about what his work reflected. Even Dylan appears more as a declining songwriter than the cult hero he always was.

In this unsentimental look at intellectual life in the 1960s, Dickstein remains consistently critical. He is hard on everybody—youth, writers, professors, even himself. His challenging generalizations and skill in tying ideas to broad cultural transition makes what could have been a mere compendium of critiques of cult writers a creative approach to the last decade. Those looking for suggestive sources or a rich bibliography, however, will be disappointed. The book is a triumph of literary and cultural criticism rather than a basic contribution to literary or intellectual scholarship. This is a personal flight and there are few footsteps to follow. The value is in the journey.

JEROME L. RODNITZKY
University of Texas, Arlington

DONALD C. LORD. *John F. Kennedy: The Politics of Confrontation and Conciliation*. Woodbury, N.Y.: Barron's. 1977. Pp. x, 458. $9.95.

John F. Kennedy's personality, ideology, and political record have, in recent years, been extensively and skeptically dissected by revisionist writers. In the preface to his new study of Kennedy, Donald C. Lord asserts that he, too, began as a "Kennedy Revisionist," but that his research led him to adopt a different stance. Lord is now an admirer of JFK; his book aims not only to retell the Kennedy story, but to restore its luster by refuting many of the revisionist criticisms.

Lord's approach blends biography and political analysis. After two chapters sketching the Kennedy family and JFK's congressional career, the bulk of his book is taken up with accounts of Kennedy's legislative record, civil rights achievements, foreign policy, and image-making activities. In each area Lord finds a few mistakes to criticize and many successes to praise. His chapter on "JFK and Black America," for example, concludes that "Kennedy handled the racial disorders

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of the early 1960s with consummate skill and kept them from getting out of hand as they did in the late 1950s and mid-60s" (p. 169).

In an interesting final chapter, "The Verdict of History," Lord divides writers on JFK into four camps. Court Historians (Sorensen, Schlesinger, *et al.*) are gently chided for their partisanship, but credited for their important revelations. Iconoclasts (Lasky, Fairlie, Clinch) are blasted for their obsession with the Kennedy image. Disappointed Idealists (Halberstam, Walton, and others) are type-cast as Stevenson liberals who never forgave Kennedy for not sharing their own illusions. Only the Political Realists (a group composed mainly of writers of unpublished doctoral dissertations and, presumably, Lord himself) see the real Kennedy. Refusing to be sidetracked by inflated expectations, they have weighed both successes and failures and arrived at a predominantly favorable judgment of the Kennedy presidency.

Lord has done considerable research, but he is not a very effective defender of Kennedy's historical reputation. His analyses largely rehash the original Kennedy chronicles, without either Sorensen's inside knowledge or Schlesinger's intellectual verve. His attempts to refute Kennedy's critics involve citing sources favorable to Kennedy or branding the critics as unscholarly and biased; the criticisms themselves are seldom addressed in detail. But the questionable aspects of Kennedy's presidential record—e.g., his penchant for crisis situations and Green Berets, the conservative thrust of his "New Economics," his efforts to dominate and limit the civil rights movement—cannot be so easily glossed over or ignored. Lord wants scholars to concentrate not on the imagery, but on the substance of the Kennedy record. His own work is so uncritical of that substance that it seems, in the end, an ironic testimony to the remaining power of the Kennedy image.

BRUCE MIROFF
University of Texas, Austin

CANADA

CARL BERGER. *The Writing of Canadian History: Aspects of English-Canadian Historical Writing, 1900–1970.* Toronto: Oxford University Press. 1976. Pp. x, 300. $12.95.

Carl Berger has written a monograph of major importance dealing with the evolution of the historical profession in the English half of Canada from 1900 to 1970. He concentrates upon the giants of the profession who were born before 1914: George Wrong, Adam Shortt, Frank Underhill, Harold Innis, Arthur Lower, Donald Creighton, and William Morton. Tangentially he includes the work of most other prominent historians who have worked in the field since World War I. Berger goes beyond the historiographical inventories, bibliographies, and essays published thus far to analyze in depth the work of the key group "who broke the traditional patterns of interpretation." Moreover, by setting these men in their context, Berger provides an intellectual and cultural history of English Canada for the seventy years involved.

In this conjunction of historiography and intellectual history Berger also makes an effort to connect the Canadian currents to related movements in Britain, the United States, and France. The separate paths which French and English Canadian historians have followed are also noted. Finally integrated into the above is a competent development of the growth of the concomitant instruments, i.e., the Public Archives and the journals.

Berger's thesis is that "Canada's historians have all been nationalists of various hues, and sometimes their judgements about what was central to the past and what was peripheral arose as much from divergent conceptions of nationality as from disagreements about interpretations of the same evidence" (p. 259). Such a statement is not strikingly original. The uniqueness derives from the application of Berger's conservative bias. This bias shows in his demonstration of the intellectual unity of each historian's work and the deep roots that even the "creative edge" (p. ix) has in the historical experience of contemporary society as well as personal experiences. Applicable to this new monograph is Northrup Frye's comment about Berger's earlier work, i.e., that he stresses the need to conserve, in the context of an ongoing examination of the question of what is necessary or important to conserve. Within the framework of this prejudice, Berger's work must be recognized as incisive and thorough, even though for a Canadian it takes some effort to appreciate a recasting of the image of such men as Underhill and Lower who have been the doyens of liberal nationalist history.

The only real criticism can be leveled at the index. A spot-check revealed numerous items that should have been included. The best explanation for this deficiency seems to lie in the conclusion that the historiographical aspect was concentrated upon while intellectual history was not given equal treatment.

Berger has done his work well, both as to research and literary style: the book is a pleasure to read. The professional is given a valuable contribution to Canadian historical literature. Nonprofessional students of history should also find

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the work intriguing, if only to appreciate more fully the history texts they used in school.

W. H. HEICK
Wilfrid Laurier University

CORNELIUS J. JAENEN. *The Role of the Church in New France.* (The Frontenac Library.) Toronto: McGraw-Hill Ryerson Limited. 1976. Pp. x, 182. $5.95.

The English language has long needed a good history of the Church in New France. The pages of Francis Parkman, a brilliant stylist, are marred by racial and religious prejudice and have the limitations of the historiography of his day. Others have written in English with less prejudice—and less charm. A serious synthesis, critically appreciative and based on archival research, has long been awaited.

Cornelius Jaenen, who moves easily between the English- and French-speaking communities of Canada, has sought to fill the need with a paperback of modest length. Previously he has studied the Huguenots of New France. Also he has focused on Amerindian cultures and on Catholic missions, to which he gives two chapters in the present work. Other chapters treat political and economic life, social welfare, manners and morals. His range of reading is broad; his search for sources is tireless. However, the book does not measure up to expectations. First, some technical points, then some conceptual considerations will show a few of the reasons for this evaluation.

The bibliography is fairly solid for a modest survey, but there are noticeable omissions. One would expect, for example, to see listed Lucien Campeau's pertinent monographs on the diocese of Quebec (1974) and on the *collège* of the Jesuits (1972). The reference system in the end notes is not fully clear or consistent. At times sources are cited precisely; at other times the curious reader looks in vain for the foundation of key statements.

In handling theological concepts Jaenen seems to have a problem. For example, he is correct in distinguishing between Jansenism and a certain school of thought within Catholicism that favored greater severity in moral teaching. However, the author has the issue confused, indeed backwards, when he calls the movement "probabilism." The strict*er* school required a person always to choose the saf*er* side, and was called probabili*or*ism, a term based on the comparative form in Latin. Probabilism, however, held that in making moral decisions a person could in good conscience judge according to a solidly grounded opinion even though it was *not* the stricter or safer side.

When Jaenen writes briefly on piety, he takes up rules and statistics on Mass attendance. But the treatment of the believers' life of worship—as distinguished from regulations and structures—should form a greater part of a history of a religious group. In Jaenen's too external view of the New France mind, we are not led beyond prescriptions on dress and drink to insights into mind and heart.

When the author does attempt to link official documents and popular practice, he trips on basic terms and meanings. He declares that "the *mandements*, correspondence and sermons of the period made only infrequent mention of the direct adoration of Jesus; the people seemed to prefer the intermediary approach through the Virgin Mary and the saints and the adoration of the Blessed Sacrament" (p. 153). For Catholics "the Blessed Sacrament" is the Eucharist, the most direct means of adoration of Jesus.

Yet the book is not "latterday Parkman," written with his hostility (or style). At times Jaenen bends over backward to extol Catholic social services and to show how Church goods were used for the sick and needy. But the tone of certain pages seems to echo some eighteenth-century Huguenot, much relieved to hear the good news that Protestant England has triumped over the French in Canada. We must continue to await an English-language work that will have technical proficiency and sensitive understanding of the Church in New France.

CHARLES E. O'NEILL
Jesuit History Institute, Rome

CAMERON NISH. *François-Étienne Cugnet, 1719–1751: Entrepreneur et entreprises en Nouvelle-France.* (Histoire Économique et Sociale du Canada Français.) Montreal: Fides, for the Centre de recherche en histoire économique du Canada français. 1976. Pp. xxxi, 185. $10.00.

One of the major questions in Canadian historiography in the last quarter century has been whether New France during the *ancien régime* possessed a distinctive Canadian *bourgoisie d'affaires* capable of providing the country with economic, and incidentally political, leadership. The question is one of importance, if only because modern-day Quebec is anxiously trying to convince itself and others that the agricultural peasant society of the nineteenth century was not the traditional pattern of French society in North America, but something imposed on it by the English "conquest" of 1763. To prove that Canada had its own golden, capitalist era in the eighteenth century, when Canadian entrepreneurs controlled the commerce of the St. Lawrence valley, would give his-

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torical justification to a modern, urbanized Quebec capable of directing its own economic and political future.

Pierre Brunet of Montreal and Jean Hamelin of Laval have been the principal protaganists in this controversy; others, such as Cameron Nish, have entered the fray from time to time. Several years ago Nish gave support to the Brunet point of view when he published his *Les Bourgeois-Gentilhommes de la Nouvelle-France, 1729–1748*, a book which went far toward establishing a plausible case for the existence of a distinctive Canadian capitalist class during the eighteenth century.

In his present contribution to this controversy, Cameron Nish zeros in on the career of one of his so-called *bourgeois-gentilhommes*, François-Etienne Cugnet. He traces Cugnet's political, juridical, and administrative career in Canada, from his arrival in the country in 1719 to his death in 1751. He also examines Cugnet's various business interests, the fur trade, stock breeding, the iron foundry, the export of tobacco, and the manufacture of glue. He establishes a good case for Cugnet, if not as a *bourgeois-gentilhomme*, most assuredly as a *bourgeois-d'affaires*, who, having gone through bankruptcy, in typical Canadian fashion succeeded in getting the government to take over his debt-ridden *forges Saint-Maurice*.

One will not fault Nish on his scholarship. His research is thorough; his documentation, impressive; his tables of statistics, convincing. His book is truly a worthwhile contribution to Canadian economic history. The only question one might raise is whether Cugnet was typical of his class and his time in Canada? We must await other books, as thorough as Nish's, before we can be certain that a Canadian bourgeoisie did exist as a class and did exercise the economic and political influence attributed to it.

There is not much missing from this book—except, perhaps, Cugnet the man. Though Nish devotes a whole chapter to Cugent the man, he adopts the approach of the social scientist rather than that of the humanist. He describes Cugnet's house, his neighbors, and his library, all nicely quantified for those who like their history in the form of percentages and tabulations. Cugnet, we learn, preferred history and belles-lettres to theology and science, when he could spare a few moments from his account books. When he died he left an estate of net value over 135,752 livres. Accepting the author's estimate that 20 livres was equal in value to one colonial pound, Cugnet's net worth was approximately 6,787 colonial pounds. Accepting, too, J. T. Mains' estimate that any man possessed of 2,000 pounds was considered "well-to-do" in the thirteen colonies, and one with 5,000 pounds was "wealthy," we can understand why Madame Bégon wrote in her journal, "*On me mande que les affaires de Mme Cugnet s'arrangent bien et qu'elle aura de quoi vivre à son aise . . .*"

GEORGE F. G. STANLEY
Mount Allison University

FERNAND OUELLET. *Le Bas-Canada, 1791–1840: Changements structuraux et crise*. (Cahiers d'Histoire de l'Université d'Ottawa, number 6.) Ottawa: Éditions de l'Université d'Ottawa. 1976. Pp. 541.

Fernand Ouellet's massive *Histoire économique et sociale du Québec, 1760–1850*, widely hailed on its appearance in 1966, in the words of the *AHR*'s reviewer, as "an event of the greatest significance in the historiography of French Canada" (72[1966–67]: 1520), gave an immense stimulus to work on the period between the Conquest of 1763 and the Union of the two Canadas. Now Ouellet has attempted a synthesis of his own research and studies of historians such as Bernard, Brun, Chabot, Galarneau, Garon, Linteau, Toussignant, and Wallot in a book which, when translated, will constitute volume 8 of the Canadian Centennial Series. Naturally enough, Ouellet returns to the thesis of his 1966 work: "The crisis of the rural economy and the population pressures building up at the beginning of the nineteenth century are the principal elements in the rural discontent which found expression first through the *parti canadien et patriote* and then in an insurgent movement led by the French-Canadian middle class" (pp. 9–10). He brings, however, several new elements of his own to this interpretation: the data and other indicators of agricultural produce, for example, which provide a more solid basis for his earlier deductions of a relative fall of production after 1805; data on demography and urban and rural social structure which illustrate the population pressures and social tensions between English and French settlers in the St. Lawrence valley.

Perhaps the greatest novelty is the space given to politics, which received only incidental treatment in the 1966 work. These chapters take the form, not of strict narratives, but of long interpretative essays, thoughtful and sometimes brilliant. A few biographical and explanatory footnotes, however, would help lead the reader through the more allusive sections; one hopes that the English-language edition will show more consideration for the reader (in passing, it should be noted that the editorial work on this volume is annoyingly slapdash). One of the most interesting aspects of the political sections is the attempt to give some notion of the actual working of elections and political machines in town and country by drawing on poll-books and local studies. Ouellet shows the persistent domination of many contests by family, inter-parish, and

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town-country rivalries, but, he contends, these were increasingly submerged by the 1830s by the larger issues of representative institutions, responsible government and independence, and by the growing tendency of voters to choose their candidates for their race rather than their principles. Unfortunately, the crucial election of 1834, in which the reforming Patriots, with many non-French candidates in their ranks, swept the boards, gets rather short shrift, perhaps because of lack of documentation. Though somewhat rough-hewn and badly in need of some simple maps of election results, these sections are suggestive and demonstrate both the possibility and interest of further research on the political sociology of the pre-1840 period. Finally, there is a dramatic chapter on the abortive revolution of 1837–38, done mainly with extensive quotation from testimony taken in subsequent trials and showing, among other things, the lamentable weakness of the Patriots' conspiratorial and military network, the chief reason, according to Ouellet, for the movement's failure.

Despite some defects of presentation, perhaps inherent in a pioneering work, this study is rich in ideas and has the double merit of being both an accessible summary of recent work and a stimulus for future debate and research.

T. J. A. LE GOFF
York University

W. KAYE LAMB. *History of the Canadian Pacific Railway.* (Railroads of America.) New York: Macmillan Publishing Company. 1977. Pp. xv, 491. $17.95.

The Canadian Pacific Railway is one of the very few North American railways which has never suffered bankruptcy, a receivership, or a forced financial reorganization. Since its incorporation in 1881 it has been one of the most important and controversial enterprises in Canadian history. The success of the Canadian Pacific Railway is in large part due to government aid in the early years and a corporate management which W. Kaye Lamb describes as shrewd, cautious, and conservative. The initial transcontinental project was a major speculative gamble, fraught with many dangers, but, having taken this first big step, the company's senior officers adopted very cautious developmental policies. This was good corporate policy, but it also led to serious criticism in those parts of Canada where rapid development and construction of new railway lines which charged reasonable rates was regarded as far more important than the payment of annual dividends. Such criticisms are certainly mentioned in this book, but the basic interpretation and orientation is that of senior management.

Lamb clearly has an interest in, and indeed a love for, equipment and rolling stock. Railway buffs will appreciate the pictures and descriptions of the company's more famous locomotives and steamships.

Much has already been written about the Canadian Pacific Railway. Lamb does not provide any surprising new information or startling new insights. He has very successfully integrated and combined, elaborated and expanded, the many bits and pieces of Canadian Pacific Railway history hitherto available to give us, in one volume, the essential outline of all facets of the company's history. In particular, the more recent history of the company and of its many affiliate undertakings is very well done.

This book will be of particular interest to those concerned about competition between private companies and crown corporations. The Canadian Pacific Railway's difficult response to competition by the government-owned Canadian National Railway is discussed in an effective and informed way. There can be little doubt that in this, as in other respects, the history of the Canadian Pacific Railways is remarkable, and Lamb knows that history well.

This is a very good, albeit conventional, railway history: the fifth volume in Macmillan's Railroads of America series. The editors and publishers of this series have done their work well and deserve praise for finding space in their series for the histories of Canada's two major railway systems.

T. D. REGEHR
University of Saskatchewan

T. D. REGEHR. *The Canadian Northern Railway: Pioneer Road of the Northern Prairies, 1895–1918.* Toronto: Macmillan. 1976. Pp. xv, 543. $27.50.

The Canadian Northern Railway, Canada's third transcontinental rail line, began as a modest Manitoba branch line in the 1890s. Although—or because—it had within a few years gone far toward becoming an integrated, profitable regional system, its promoters, owners, and principal contractors, William Mackenzie and Donald Mann, determined to create a transcontinental system. The sharp economic downturn after 1912, World War I, and the massive capital requirements of the project, however, led the company into insolvency and, by 1918, government ownership. It is on this company, more than the northern prairies, that T. D. Regehr focuses. His perspective is that of the company's more ardent partisans: thus the accounts of the company's labor relations and its subcontracting system, for example, verge on the disingenuous, while the account of its takeover by

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the government glosses over the reality of its bankruptcy, measured by the central test—its inability to meet payments.

Despite a few revealing anecdotes, notably about Mackenzie, neither promoter emerges very clearly as a character whose vast ambitions we can begin to comprehend. The book portrays them, rather surprisingly, more as important western figures than as capitalists. Of course they built the Canadian Northern and western settlement grew with it. But is it possible that, in an imaginary historical world that lacked the two great entrepreneurs, no railroads would have been built on the northern prairies, or that settlement of that territory would have been long, let alone harmfully, delayed? It is somewhat disappointing that many fundamental business decisions are not more fully analyzed. Why did Mackenzie and Mann choose to finance the company by bonded and short-term debt, reserving the entire equity for themselves alone? The control they gained was offset later by financial inflexibility and by the reputation for profiteering it inevitably engendered. Did they not consider such issues?

Such questions are not meant to disparage, but to set the book in its context, which is, above all, that of Canadian political history. Strong provincial ambitions, divisions within the federal Liberal Party, and the preferences of Sir Wilfrid Laurier himself are shown to have permitted Mackenzie and Mann to pursue their transcontinental dreams and to justify their claim that they had no choice but to do so. The book is, therefore, an indispensable work on complex issues that lay very close to the heart of Canadian politics in the early twentieth century. In an admirably produced book a few errors have still crept in; for example, the Canadian Northern and the Grand Trunk Pacific-National Transcontinental are confused on the map on page 103, and scales have gone awry on the maps on pages 128 and 140.

DOUGLAS MCCALLA
Trent University

NEIL SUTHERLAND. *Children in English-Canadian Society: Framing the Twentieth-Century Consensus*. Toronto: University of Toronto Press. 1976. Pp. viii, 336. $19.95.

This informative but limited study traces the development of charitable, correctional, educational, and health services and institutions for some of the infants, children, and youths of English-speaking Canada. Neil Sutherland has mined dominion and province records, church magazines, institution and school reports, papers and publications of voluntary associations and notable "child-savers," health surveys, and yearbooks to demonstrate that a broad concern about the welfare and control of children helped to shape the welfare state in Canada. Scholars pursuing particular topics will find the author's "Bibliographic Note" and explanatory footnotes particularly useful.

Sutherland documents several important changes in English-Canadian attitudes and policies toward children. These include: 1.) the emergence of manual training and vocationalism in public education; 2.) the shift from institutionalism to home-centered services for orphaned, dependent, and delinquent children; and 3.) the development of compulsory national standards for infant and child health care, as well as for school attendance, curriculum, and teachers' qualifications. These changes were the work of a network of urban middle-class men and women operating through voluntary groups such as the National Council of Child Welfare and the Women's Christian Temperance Union. Reformers lobbied successfully for the legislation that enabled them to implement their ideas in rural areas. Sutherland analyzes this reformism in a comparative context, noting particularly the tensions between British and Canadian children's aid societies. He also emphasizes the numerous and friendly contacts between American and English-Canadian children's organizations.

Overall, Sutherland gives his reformers high marks. He views their work as conducive to the general welfare because it was rooted in the discovery of children as individual personalities with special needs. He deplores certain programs such as the "Canadianization" campaigns against the language schools of immigrants from Central Europe and the sterilization enthusiasm of early mental hygienists; but these are portrayed as exceptions to a broadly supported movement benefiting large numbers of children.

Sutherland's generally optimistic conclusion must be seen as part of his effort to establish a historiographical middle ground within the history of education. In an introductory essay in Michael B. Katz and Paul M. Mattingly, editors, *Education and Social Change: Themes from Ontario's Past* (1975), he has sought to position himself between traditionalists like Ellwood P. Cubberley, for whom the growth of public education represented the march of progress, and contemporary critics such as Katz who have portrayed the advent of compulsory schooling as one tool employed by local and national elites to protect and further their own hegemony. In *Children in English-Canadian Society*, Sutherland has not fully established the validity of his view because his analysis proceeds in a partial vacuum. He locates much reformist sentiment in Ontario. But the reasons for reform activity there

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go beyond the challenges posed by rapid economic growth and urbanization. Here more than anywhere, Protestant English-Canadians were faced with the potentially threatening prospect of having to share power with Irish-Catholics and French-Canadians migrating from Quebec. Sutherland neglects the impact of these groups and their philanthropic and educational systems in shaping English-Canadian reform ideology. Traumatic conflicts such as the Ontario School War of 1915, in which bilingualism was sharply rejected, are not mentioned. The author does not purport to analyze the place of French-Canadians or Irish-Catholics in Canadian society, but he is remiss in not relating the quantity and quality of English-Canadian benevolent activity to the challenging presence of these groups. By ignoring substantive social and political conflict, Sutherland dilutes the moderate stance and thus remains closer to Cubberley than he believes himself to be.

ROBERT M. MENNEL
University of New Hampshire

CHARLES M. JOHNSTON. *McMaster University*. Volume 1, *The Toronto Years*. Buffalo, N.Y.: University of Toronto Press, for McMaster University. 1976. Pp. xvi, 295. $15.00.

McMaster University was chartered in 1887 and was established in Toronto as a Baptist institution. This first volume of its history covers the years when it struggled to maintain itself in that city. An early founding spirit was John Gilmour, seaman, schoolmaster, and Baptist minister. It was William McMaster, however, who turned Gilmour's dream from a starveling denominational school into a university and during its founding years stood by it as father protector. The course of McMaster University in Toronto was a difficult one, in which the future almost seemed to be one of certain disaster.

Reflected in this book are many of the woes and uncertainties which characterized private denominational higher education in all North America. In many respects this Canadian experience parallels that of hundreds of such tiny church-related colleges across the United States. The history of McMaster was beset by problems of friction within the faculty, the organization and maturing of a curriculum, providing even the most meager financial support, and serving a minority Calvinistic denomination. Deeply tincturing the McMaster story were the conflicts and internal rivalries among staff members and among the Canadian Baptists. There was no precise notion of what a religiously related education should consist of at the college level. World War I threatened McMaster with further setbacks, and its after-effects continued down into the late 1920s. In October 1927, the board of trustees voted to move the institution to Hamilton where it could make a fresh start.

Charles M. Johnston has been a diligent scholar in his factual researches, and he has published his findings in generous measure. Throughout his book he demonstrates a remarkable patience with a group of dedicated but willful men who approached their varied roles as zealous missionaries. This work is well documented, well printed, and ample in text. The text is conscientious but lacking in those human vignettes which have always characterized the growth of a university. The faculty was as sternly proper as a Scots meeting of a corporate board. There are no chapters which describe the routine of classes, the nature of the students, or any other kind of collegiate color. The Canadian Baptists were as dour as many of their American brethren, and higher education was a godly-ungodly undertaking for them. Thus Johnston has presented them in his history of their struggles and anxieties. This meticulous study has a staid comparative value in the historical literature relating to the founding and nurturing of North American universities.

THOMAS D. CLARK
Lexington, Kentucky

LATIN AMERICA

WILLIAM M. DENEVAN, editor. *The Native Population of the Americas in 1492*. Madison: University of Wisconsin Press. 1976. Pp. xxii, 353. $15.00.

No one seriously interested in the history of the Americas can avoid the controversy over the size and distribution of the native population at the time of discovery. But as this book so neatly illustrates, few historical problems leave the non-specialist with such a sense of frustration and exasperation. Indeed, it is testimony to the significance of the controversy that so many scholars have struggled so valiantly to bring order out of the bewildering array of data, estimates, hypotheses, and methodologies that constitute the debate.

The argument, of course, is not simply one of numbers, statistics, mathematical models, or ecological analyses, but involves our understanding of the process of European conquest, settlement, and expansion in North and South America over at least three centuries. No small matter, that. Reduced to its most simple form, the historical problem can be stated in this fashion. If there were only a few hundred thousand or at most, perhaps a couple of million, Indians when Columbus landed,

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then the subsequent decline of the native populations and the reorganization of Indian society can be viewed as fascinating, but minor, facets of the brilliant European venture in America. If, in contrast, something on the order of fifty or sixty million Indians lived in the Americas, then the decline of the native population and the reorganization of native society take on an entirely different character. In the first case, we may be talking about incremental changes, cultural adaptations, and gradual social processes. In the second case, we may be confronted with catastrophic mortality, virtual genocide, total cultural destruction, and massive ecological and social transformations. Clearly, then, the argument is worth pursuing.

The difficulty with the debate, especially from the perspective of the historian without personal experience with the documents, comes from the peculiarly tenuous nature of the material available on preconquest populations. What is there to go on? Well, there are the reports of the first explorers and conquerors, the estimates of the early missionaries, the first tribute censuses, and a few other similar documents. In addition, there are extensive archeological remains, a wide variety of ecological considerations, and a vast array of medical insights. In varying degrees, the scholars represented in this volume worked with all of these materials. And in spite of their expertise, their long experience with the sources, and their control of sophisticated methodologies, they end up disagreeing by rather wide margins. Although the volume includes a piece by Ángel Rosenblat, the most persistent and best known defender of low estimates, the recent trend of scholarship is toward high estimates.

All of the contributors to this volume have given exceptionally clear and comprehensive essays which give a convincing representation of the state of the art. Woodrow Borah has an outstanding discussion of the history of the debate which clearly shows the development of ever higher estimates. This masterful survey by the best known and most prolific of the scholars in the field is followed by a series of studies focused on specific problems or geographic areas. Rosenblat has a fascinating piece on the population of Hispaniola in 1492; David R. Radell makes estimates for the population of Nicaragua in the sixteenth century with special emphasis on the Indian slave trade; William T. Saunders presents a detailed critique of the Borah, Cook, and Simpson estimates for Central Mexico and gives a figure about half of theirs on the basis of an analysis of archeological evidence, re-evaluation of Spanish documents, and a careful discussion of rural-urban ratios; Daniel E. Shea describes a useful methodological procedure for estimating preconquest populations and applies it to the Central Andes to derive relatively small numbers; Jane Pyle revises Argentine aboriginal population figures upwards; William M. Denevan makes a persuasive case for a dramatic upward revision of estimates for Amazonia; and Douglas H. Ubelaker evaluates James Mooney's estimates for North American Indian populations to demonstrate that they represent a minimum conservative estimate.

This catalogue of the various contributions in the volume hardly begins to indicate the richness and fascination of the collection. Much of the utility of this volume, however, comes from the exceptionally useful, reasonable, and intelligible introductions and epilogue provided by the editor, William M. Denevan. Combined with Borah's survey of the field, Denevan's material gives any interested reader a good understanding of the debate.

Many of us may find the arguments difficult to follow in some of the essays, for they build on chains of deduction, documentary evaluation, and ecological analysis accessible only to the expert. Moreover, it is no simple matter to decide which estimates seem most reasonable. Each expert in this volume uses different assumptions, different models, different methods. And each presents a remarkably convincing argument in support of these methods, models, and assumptions. Reconciliation of the conflicting visions of the American continents before Columbus is still a long way off.

The bottom line on the aboriginal population of the Americas is, at least at this reading, somewhere between 43 and 72 million with a few scholars well above this and some way below. Because this book is such an outstanding contribution to our understanding of the debate, no one professing expertise on the American past should be allowed to practice without having studied these essays.

JOHN V. LOMBARDI
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JOSEPH W. WHITECOTTON. *The Zapotecs: Princes, Priests, and Peasants*. (Civilization of the American Indian Series.) Norman: University of Oklahoma Press. 1977. Pp. xiv, 338. $14.95.

Joseph W. Whitecotton attempts a broad synthesis of the social and cultural history of Zapotec-speaking peoples in southern Mexico. The book seems to be directed at an audience somewhere between the professional and the lay reader. Therein lies one of its weaknesses. The specialist will criticize it for glossing over problems and overgeneralizing. The popular consumer will find some sections painfully detailed. Perhaps undergraduate college students taking an area course in Mesoamerica will be the most receptive audience.

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The writer begins with a brief account of hunters and gatherers in the Valley of Oaxaca and brings us into contemporary times, some 10,000 years and three hundred pages later. Naturally we have no idea what language those early food collectors spoke and archeologists generally do not write of a Zapotec tradition until Formative times (ca. 1500–500 B.C.). This tradition crystallized in Classic times with the founding of the great center of Monte Alban around 500 B.C. Use of the term "Zapotec" may refer to a people speaking related dialects of a language, an ethnic heritage, or a cultural tradition. Whitecotton touches upon this problem, stating that the designation "Zapotec' has generally been "more of an artifact of external observers than a meaningful unit for the people to whom it has been applied."

The first part of the book—particularly the section dealing with Formative times in the Valley of Oaxaca (chapter two)—suffers from the omission of recent research findings for that developmental period. The work of Kent V. Flannery and his colleagues (see *The Early Mesoamerican Village* [1976]) is crucial to our understanding of the formation of early towns and villages in Oaxaca and other areas of Mesoamerica. This lacuna is almost certainly not Whitecotton's fault. The manuscript was apparently delayed several years before reaching print.

The strongest parts of the book are chapters three and four, which deal with political and social relationships in post-Classic Zapotec culture and society. Chapter five, which treats Zapotec elites and peasants in New Spain, again suffers from lack of inclusion of contemporary research, especially John Chance's work on Colonial Oaxaca.

The final chapter, "The Zapotecs in Modern Mexico," dwells too much on generalizations and basic anthropological concepts: the nature of peasantry, how market systems operate, foundations of political and religious organization. The author draws heavily upon George Foster's models to explain peasant behavior—the Image of Limited Good, the Dyadic Contract, the Equilibrium Model—with little critical evaluation. Along these lines, Whitecotton does not always separate the real from the ideal behavior of peasants: "A woman must always be submissive to her husband." Anyone who has spent time in Mitla, for instance, *knows* this is not so. This chapter is, though, a useful general introduction to peasant life in contemporary Mesoamerican villages.

A glossary would assist the nonspecialist—particularly since the author not uncommonly introduces a term without defining it until later—and might also have prevented some careless errors. He mentions "the distillation of *pulque*." *Pulque*, of course, is a fermented, not a distilled drink.

Despite its shortcomings, Whitecotton's work is a useful summation of our knowledge of the Zapotecs. The plentiful photographs add much to the volume's appeal. It is unfortunate that the author himself did not contribute more original research to the subject matter.

DOUGLAS BUTTERWORTH
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HANS POHL. *Studien zur Wirtschaftsgeschichte Lateinamerikas*. (Wissenschaftliche Paperbacks, Sozial- und Wirtschaftsgeschichte, number 10.) Wiesbaden: Franz Steiner Verlag. 1976. Pp. viii, 287. DM 18.

In deference to his teaching tasks, Hans Pohl devoted time intermittently over a fifteen-year period to the preparation of this collection of random studies concerning Latin American economic history between the sixteenth and early twentieth centuries. He relied essentially on his doctoral dissertation; some initially unrelated research in the archives of a Cologne business firm; material that he utilized for a lecture before the Austrian Institute for Latin American Affairs; the guidance of his teacher, the distinguished Latin Americanist Richard Konetzke; a series of findings that emanated from an international congress of Americanists in Stuttgart; and, finally, the contributions of some of his own students.

These disparate investigative approaches offer a clue to the source of the fundamental malady from which this work suffers. A vague title theme encompasses a structure of such meaninglessly broad topics as "History of Commerce and Communications" and "History of Agriculture," which fail to consolidate wildly diverse chapter discussions. In addition, footnotes are often of a superficial or slipshod nature. Furthermore, only one chapter in the entire monograph includes a bibliography, albeit without potentially helpful annotations. Especially in view of these flaws in scholarship, the sometimes harshly pedantic style of writing, as well as some lengthy recitations of proper names and statistical material of dubious importance, constitute incongruous elements in this study.

Though sometimes brief, the essentially self-contained individual chapters frequently furnish trenchant summaries and significant descriptive information or pose provocative questions that encourage further research. The first chapter, which traces the evolution of the *consulado*, or colonial merchant guild system, and which also refers to significant investigations concerning this topic, is presented lucidly. But it is followed by a rather cursory treatment of smuggling activities during

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the colonial period. By contrast, the third and fourth chapters offer more exhaustive studies, namely the port of Antwerp's participation in the Latin American sugar trade and the involvement of the German Hanse towns in Latin American commerce in about 1800.

With typical abruptness, events of compelling interest that led to the laying of the first German overseas cable to South America are discussed next. And, again, the painstaking quality of this investigation overshadows a subsequent shallow treatment of urban employment opportunities in colonial Spanish America. The remaining chapters embrace meaningful discussions of the textile industry in colonial Puebla, the industrial and agrarian evolution in Mexico under Porfirio Díaz, and, strangely in last place, the nature of economic enterprise engaged in by the Spanish American aristocracy during the seventeenth and eighteenth centuries. These last chapter texts also include several useful references to pertinent scholarly literature.

WARREN SCHIFF
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MARK A. BURKHOLDER and D. S. CHANDLER. *From Impotence to Authority: The Spanish Crown and the American Audiencias, 1687–1808*. Columbia: University of Missouri Press. 1977. Pp. xii, 253. $15.00.

The recent use of statistical data and quantitative techniques by colonial Latin American historians has permitted a more precise definition of the social, commercial, and financial structure of the Spanish American empire. This quantitative analysis of Spain's changing policy of appointing judges to the eleven American *audiencias*, or royal high courts of appeal, nicely complements the earlier works of Ernst Schafer and Richard Kagan by helping to define the administrative structure of the Indies.

The book is based on a statistical assessment of the 693 *oidores*, or judges, appointed to the American *audiencias* between 1687 and 1821. Although approximately two thousand men served on these courts from their inception in 1511, Mark A. Burkholder and D. S. Chandler focus their study on those appointed in the years following Charles II's decision to sell these judgeships. During this time royal control over the courts rapidly crumbled, ushering in an "Age of Impotence" that ended only with the advent of Bourbon regalism after 1750. Prior to this time, the emphasis placed upon social and economic background, particularly attendance at certain major Spanish universities, tended to favor Peninsular, as opposed to American-born creole appointees. After 1687, however, creoles began to obtain judgeships in increasing numbers, many of them "native sons" who were appointed to the bench in their districts of birth. Others developed networks of interests and developed connections based on consanguinity and marriage, becoming *radicado*, or "rooted" to particular locales.

Although the later Bourbons never formally established a policy excluding creoles from service on the American *audiencias*, the authors' analysis of *audiencia* appointments made after 1750 indicates the existence of a *de facto* policy by 1776. In an apparent effort to reestablish and maintain royal authority in the Indies, the Bourbon monarchs and their zealous ministers, notably José de Galvéz, replaced native sons and *radicado* judges with experienced Spaniards who more nearly conformed to the ideal of "Platonic guardians." By 1808 only twenty-five creoles remained among the ninety-nine men serving on the American courts and of this number only six were native sons. Without the "safety valve" effect afforded by the former Hapsburg policy, creole requests for increased representation mounted as part of a general demand for home rule. In the wake of the Napoleonic invasion of Spain in 1808, the Spanish judges on the *audiencias* predictably became the earliest casualties of the independence period.

By clearly delineating the pressures and actions that contributed to the evolution of royal policy governing *audiencia* appointments and developing a series of career biographies of appointees, supported and illustrated by useful statistical charts and graphs, Chandler and Burkholder point the way toward the collective biographical history of the Indies bureaucracy. Case studies of particular courts can tell us precisely how creoles responded to these policy changes and can perhaps explain why creole influence was allowed to persist longer in Lima and in Chile than elsewhere. It would, for example, be useful to place the 693 appointees in the larger context of the *letrado*—the lawyer bureaucracy—and of the creole social community, in order to address questions concerning the overall growth of the government under the later Bourbons, the shared characteristics of their administrators, and the linkages between population growth and demand for appointments. Answers to these and other questions, however, are closer as a result of the appearance of this scholarly and well-written book which substantially advances the field of colonial administrative history.

LEON G. CAMPBELL
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PEGGY K. LISS. *Mexico under Spain, 1521–1556: Society and the Origins of Nationality*. Chicago: University of Chicago Press. 1975. Pp. xvi, 229. $12.50.

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With this short book Peggy Korn Liss makes an important contribution to the understanding of early Mexican history, especially the crucial twenty-five years following Cortés' defeat of the Aztecs. Although the book is not the product of research in original sources—in her own words it is "somewhat of a massive brush-clearing" (p. xi)—she has surveyed all of the relevant printed materials to show how the historical perception of conquerors and kings helped shape the course of subsequent events and attitudes in Mexico.

The Catholic Monarchs, Ferdinand of Aragón and Isabella of Castille, changed Spain from a congeries of seignorial and municipal interests to a pluralistic and patrimonial national state, by curbing dissident nobles, priests, and townspeople and imposing royal officials (corregidores), responsible and loyal to the crown. Yet at the same time the wars of reconquest against the Moors created a national mystique that exalted the warrior and military ideals. The conquerors, of whatever class, brought with them visions of the Christian knight, warring against Devil and infidel—and of course they all hoped to get rich in the process! For their pains they would become landowners, holding encomiendas of Indians to work the hard-won land. In a word, they yearned to be seignorial masters in America. Understandably, the Spanish monarchs, first Ferdinand and Isabella and later their Hapsburg grandson, Charles V, resisted such seignorial pretensions. Destroying feudalism in Spain, the kings were not about to allow the growth of seignorial institutions in the Indies. Thus Mexico saw in these early decades a protracted and at times bitter struggle involving the crown and royal officials, on the one hand, and the conquerors and their sons, on the other.

Liss shows how the heated debate at Valladolid between Bartolomé de las Casas and Juan Ginés de Sepúlveda fits neatly into this political conflict. If Sepúlveda were held correct, if the Indians were indeed "natural serfs" (the author errs in using the word "slaves" here), then some sort of feudal relationship was inevitable. The crown, therefore, could not permit Sepúlveda's theories to prevail. The encomienda became only the right of Spaniards in the New World to collect tribute from the Indians, not to govern them as feudal lords. Indians and Spaniards in the Indies were ruled by royal officials, by viceroys, oidores, governors, and corregidores.

The crown's victory was far from complete, however. Liss does not deal with the long period after the accession of Philip II in 1556. But she demonstrates conclusively that already, by the middle of the sixteenth century, the Spanish genius for compromise had prevailed. The death of the viceroy in Peru and the wise counsel of Antonio de Mendoza in Mexico showed that even powerful kings must bend before the winds of protest. In the end, through the institution of the hacienda, a kind of seignorial relation emerged, despite the crown's intentions. Though the last viceroy returned to Spain during the Wars for Independence, the seignorial regime persisted in Mexico's countryside until the great Revolution of 1910.

ROBERT E. QUIRK
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JEAN A. MEYER. *The Cristero Rebellion: The Mexican People between Church and State, 1926–1929.* Translated by RICHARD SOUTHERN. (Cambridge Latin American Studies, number 24.) New York: Cambridge University Press. 1976. Pp. xi, 260. $19.50.

This is a courious work: fascinating yet frustrating, a mix of some probing research with some careless citations, a blend of cautious generalizations and passionate partisanship. Even its publication history is odd, as Jean Meyer himself notes. Originally a thesis of massive proportions, it appeared first in a three-volume Mexican edition (1973–74), then in a French edition consisting of documents and commentary (1974), and penultimately in a second French edition (1975) roughly approximating this present English edition. But there is more: Cambridge University Press was the first to offer Meyer a contract to reduce his huge manuscript to publishable proportions. Consequently the author tells us that this is properly the first book, a complete rewrite of the thesis, "and that what one has here is an original text, and not the translation of a French book" (p. ix).

Meyer is at his best in the very core of his work—describing and interpreting the Cristero movement in terms of its people, its organization, its activities. He has used a wide variety of sources and documents from participants and sympathizers including oral interviews with survivors. One comes to understand if not always to sympathize with the feelings and emotions that led to rebellion.

On the other hand the author becomes so enamored of his Cristeros that he loses perspective and becomes at times more polemicist than historian. At the very end of his work he seems to recognize this weakness, maintaining that "neutrality was impossible," that he "refused to be caught in the trap of the official mythology," and that he had "begun with a personal point of view hostile to the Cristeros" (p. 218). All well and good, but unquestioning acceptance of heroic death stances (see p. 192), superficial treatment of anti-Cristero groups,

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and a running apologetics for the object of his study enormously weaken a potentially significant work.

Let me cite two other distinct but related problems: one of perspective and the other of citations. In a bare thirty-two pages Meyer tries to place the Cristero uprising in the context of the broad sweep of Mexican history. He offers some striking and controversial interpretations of Mexican developments from the conquest to the Revolution: the defeat of Bourbon regalism in 1821, the clericalism of the common people versus the anti-clericalism of the ruling elites in the nineteenth century, the all-pervasive role of the United States, the importance of Protestantism and freemasonry in the Revolution, and the reformist nature of the Catholic Church and bishops after 1900. Unfortunately, he neither presents much evidence nor cites appropriate studies to support these positions. Secondary sources listed in the bibiography are limited to begin with, and scarcely cited in the footnotes. There is a serious error on page 248 where he lists in the bibliography Robert Quirk's 1950 doctoral dissertation "The Mexican Revolution and the Catholic Church, 1910–1929." He then notes a "1st ed., Indiana University Press, 1960; 2nd ed., New York, 1963, paperback." There are no such editions. Quirk's dissertation *was* published by Indiana University Press, but not until 1973.

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MERILEE SERRILL GRINDLE. *Bureaucrats, Politicians, and Peasants in Mexico: A Case Study in Public Policy*. Berkeley and Los Angeles: University of California Press. 1977. Pp. xix, 220. $12.50.

This excellent monograph focuses on the operations of the Mexican federal agency CONASUPO during the first five years of the Echeverría administration (1970–75) in a case-study approach to the dynamics of the Mexican political system. For the past several years many North American observers have attempted to penetrate the shroud of ambiguity which cloaks the ostensibly simple but essentially highly complex one-party system of Mexico. Investigators have attempted especially to determine the locus of power in the decision-making process. Early on this was pursued by examining the governmental structure for clues. Unclear results, thereupon, shifted interest to the official party, PRI, with equally unclear findings. More recently attention has been focused on the existence of informal exchange alliances or cliques based on intense personal loyalties which pervade the government, the party, and the bureaucratic-administrative structures. This study by Merilee Serrill Grindle represents a significant contribution to this literature and a welcome assimilation and refinement of previous theories.

In addition, the author is to be congratulated, for, despite her training as a political scientist and her extensive contact with the bureaucracy, she admirably resisted, for the most part, lapsing into the numbing jargon of either.

Although an exhaustive review was undertaken of the secondary sources on Mexican government and politics, the real contribution of this study comes from extensive interviews by the author with over seventy-eight middle- and upper-level administrators within CONASUPO and many other interviews with Mexican academic observers and administrators in other agencies. The information from these sources is not verifiable, because anonymity of the informants had to be maintained due to the sensitivity of the subject. The author seems, however, to have conducted the interviews with skill, and her evaluations and interpretations seem both judicious and objective. Three noteworthy conclusions are reached. First, the bureaucracy not only implements policy in Mexico but is a key element in policy formulation. Second, the *sexenio*, or six-year turnover coinciding with the presidential succession, has a tremendous influence on policy-making and the ability of the system to effect economic and social change. Third, the key to understanding the operation of the Mexican political system is the personalistic patron-client relationship among the participants which results in the formation of cliques or camarillas structured pyramidally. The real dynamics take place within these groupings.

A bonus benefit of this book is the insight it gives, albeit obliquely, into the orientation of the Echeverría administration as it tried to use CONASUPO as an instrument to effect rural development.

HUGH G. CAMPBELL
California State University, Chico

JEAN PIEL. *Capitalisme agraire au Pérou*. Volume 1, *Originalité de la société agraire peruvienne au XIX^e siècle*. Paris: Éditions Anthropos. 1975. Pp. 330.

The weight of Jean Piel's study, of which the present volume is merely a survey of the background, is to fall on the great agrarian estate in Peru from the mid-nineteenth century to 1920–30. One can render no full judgment on the first volume while the second, the core of the work, is still unavailable. The introductory portion under review here makes little original contribution in itself. Many

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pages are devoted to trying to give the general reader some notion of the history of man in the Andean area. Successive sections treat agrarian and over-all societal developments in the pre-conquest, colonial, and independence periods. One recognizes in the author both intelligence and a quite doctrinaire, twentieth-century-oriented attitude, but neither of these factors comes fully into play. The value of the presentation is simply that of the previous scholarship on which it rests, lacunae being filled with whatever stereotypes are current in Piel's circles.

By virtue of a reliance on John Murra, whose excellent work has shown the Inca empire not as a monolith but as a structure building on locally autonomous societies, the pre-Columbian section is quite good.

The section on the colonial period is far less satisfactory. Only for the latter eighteenth century does Piel have a solid base, the work of Pablo Macera on Jesuit haciendas. Piel is entirely unaware of a school of seven or eight writers in English who have transformed Peruvian colonial history over the past decade. He relies to some extent on the model of François Chevalier's work on the Mexican hacienda, but again is unaware of a school of historians who have revised Chevalier's conclusions. The presentation is highly unintegrated. Piel makes the standard older assumption of a monopoly position for the hacienda, then reports the lack of such a position for the eighteenth century, then proceeds to use it as the basis of conclusions anyway.

A final section on the independence period is largely limited to discussion of decrees and legislation concerning landholding.

JAMES LOCKHART
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PEDRO-PABLO KUCZYNSKI. *Peruvian Democracy under Economic Stress: An Account of the Belaúnde Administration, 1963–1968.* Princeton: Princeton University Press. 1977. Pp. xiv, 308. $16.50.

In recent years there has been a general upsurge in interest in Peruvian affairs in both the United States and Europe. This interest blossomed in the wake of the military "revolution of 1968" which inaugurated the most sweeping agrarian reform in Latin America since the Cuban revolution. And it culminated in the appearance of a number of books in English which have illuminated the flow of recent Andean history and politics—Tullis and Handleman on the peasant movements of the 1960s, Goodsell on American business in Peru, and Fitzgerald and Lowenthall on the "military revolution of '68," to name only a few. These works are now joined by that of Pedro-Pablo Kuczynski, an economist by training, who seeks to examine the "prerevolutionary" regime of Fernando Belaúnde Terry, a moderate reformer, democrat, and sometime visionary who governed Peru during much of the previous decade and who was overthrown by the military in 1968.

The appearance of Kuczynski's book is quite timely since current forces are moving Peru slowly back from a period of "revolutionary authoritarianism" toward a "democratic" future in which Belaúnde himself may come to play a significant role. Moreover, the author, having been closely involved in decision-making at the time as part of the managerial team of Harvard-educated "whiz-kids" at the *Banco Central*, both chronicles and analyzes the economic crisis, not all that dissimilar to the present, that led up to the 1968 coup. Unfortunately, no overtly startling revelations or conclusions are presented in this book. Rather it takes the form of historical memoir, an intelligent chronicling of events, which, although containing some analysis, does not really draw the larger meaning from these rather convulsive times in Peru.

Many Peruvianists, including this reviewer, believe that Belaúnde squandered a superb opportunity, perhaps because of his penchant for scrupulously observing democratic forms, to radically recast the archaic and feudal structures of Peruvian society. Kuczynski agrees that opportunities were lost, but not on the side of radical reform. Rather, reflecting a somewhat progressive variant of the technocratically oriented Brazilian model for Latin American development, he believes that Peru lost an excellent opportunity for sustained economic growth and even take-off. This could have been achieved, Kuczynski implies, had the president not only toned down some of his reform initiatives, but also listened more intently to his economic advisers (presumably from the Central Bank), who were arguing the urgency of immediate tax increases and an end to price controls on basic staples, than to his political advisers.

The author may well be correct about such growth potentialities. A focus on accelerating growth, however, doubtless would have been achieved at the expense of urgently needed structural reforms, perhaps even postponing indefinitely the agrarian reform which finally came not from Belaunde's government, which had promised it, but from its military successor and which, at long last, liquidated Peru's feudal agrarian past. Kuczynski implies that such economic growth might well have preserved Peruvian-styled democracy, but in a society of such deep-rooted social inequities, widespread poverty, and illiteracy

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would that not have represented a luxury the majorities could neither afford nor, at bottom, tolerate?

PETER F. KLARÉN
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JAMES R. SCOBIE. *Buenos Aires: Plaza to Suburb, 1870–1910.* New York: Oxford University Press. 1974. Pp. xvii, 323. $12.50.

"Pearl of the Pampa" or "Goliath's Head," the city of Buenos Aires has overshadowed Argentine history to the point that—for good or for ill—its development has been difficult to separate from that of the nation as a whole. This surely was the point James R. Scobie wished to make, by indirection at least, in the title of an earlier work, *Argentina: A City and a Nation* (1964). In this major new monograph, at once a contribution to both the history of urbanism and of Argentina, he explores the theme explicitly and in fascinating detail.

After a brief review of the city's history throughout a long and largely quiescent "colonial" period, Scobie then offers us two contrasting pictures—snapshots, really—one of the city as it appeared on the verge of "take-off" in 1870, another four decades later, when the process of "modernization" was held to be complete. Four subsequent chapters treat locational forces in urban growth, housing, transportation, and social and cultural development, building a kind of theoretical bridge which explains how a squat cattle town of the seventies could come to be regarded as "the Paris of South America" on the centenary of Argentine independence in 1910. The final chapter, "The Commercial-Bureaucratic City" argues for a special category for Buenos Aires in the history of urban development, a singularity which, again, is congruent with that of Argentina as a whole, a country long regarded as a variant model among Latin American nations.

Perhaps, indeed, there are cities which, like Topsy, just grew, but Buenos Aires does not appear to have been one of them. If there is one theme which Scobie stresses above all others, it is the significance of conscious choices made by the Argentine elite, choices which shaped (or disfigured) the development of a great city and its inhabitants. The centrality of Buenos Aires in Argentine history has always rested upon its role as the principal port of egress for the nation's agro-pastoral production, and in the past two hundred years, the major point of entry for a wide variety of foreign goods and European immigrants. In this context such questions as where to locate the port works became major issues of economic orientation, and so mundane a matter as tramway fares could determine whether or not an enormous (and often desperately poor) immigrant working class could afford to play the role in Argentina's economic development already determined for it by the nation's privileged classes. To demonstrate these points Scobie has plunged into national and municipal archives, and exhumed the long-forgotten debates which each issue of urban planning invariably raised.

Historians ritually repudiate what Herbert Butterfield once called the "Whig interpretation of history," but how few practice what they preach! This is decidedly not a deficiency of Scobie, who demonstrates repeatedly that alternative paths to urban development rejected along the way can shed as much light on the social and political history of a city as the familiar highways to the present.

If one has a particular reservation about this book it is that—like most ground-breaking studies—it tries to do too much. So many fascinating questions are raised, and then, irritatingly, left to dangle as the author moves on to other concerns. And at times the boundaries of inquiry are hopelessly vague—particularly chapter six ("Social Structures and Cultural Themes"), which rehearses much information already known to specialists, and whose precise relevance to the major theme—except in a general way—is sometimes unclear. Yet this much said, rarely have matters of sewage, transportation, housing, and so forth, been presented in so absorbing a manner. This is a fascinating book, and at the same time a deeply human one. One's admiration goes out to those Argentines, Italians, Spaniards, Jews, and Englishmen, whose hard work and faith built a great city. Excellent maps and well-selected photographs vivify the narrative. For those who already know Buenos Aires and Argentina, Scobie's book will add immeasurably to their knowledge and appreciation; for those who do not, it will awaken a sympathy and an interest. And for urban historians of all countries, it represents an important and indispensable contribution.

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Communications

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TO THE EDITOR:

" 'Made for Man's Delight': Rousseau as Antifeminist" (*AHR*, 81 [1976]: 266–91) tries to relate Rousseau's views of women to his work as a whole in order to provide a balanced understanding of Rousseau's antifeminist positions. Victor G. Wexler, however, fails to realize his goal, in part because he emphasizes only Rousseau's autobiographical writings and their relations to *Emile*. Thus, Wexler leaves unresolved the original paradox: Rousseau as revolutionary, Rousseau as antifeminist. If Rousseau's position on the education of women in *Emile* is linked to his political theory, then the paradox can be explained.

The few references to women in the *Social Contract* and the *Discourse on the Origin of Inequality*, combined with those in *Emile*, make clear that the abstract citizen of Rousseau's political theory is concretely male. This citizen participates in political relations among autonomous, relatively rational individuals brought together, in Rousseau's theory, out of social necessity. But, though the political sphere is crucial for Rousseau, society is not reducible to politics. The main social institutions are connected through the activities of the citizenry; those institutions, however, are not subsumed under a single principle (such as "equality"). The citizen is also economically independent and the head of a family (see, for example, the *Projet de constitution pour la Corse*); neither economic nor family life is dominated by the principle of equality.

Rousseau's political radicalism and his antifeminism are therefore fused in the concept of a virtuous society composed of independent families, integrated politically through their male heads. Rousseau relentlessly attacks relations of intimate dependence at the political level; a virtuous polity, in his view, is quite austere. Relations of dependence, however, do not disappear completely from social life, but continue to flourish within the family. Rousseau's fear of women and his contempt for women are not exclusive; both are present within a coherent conception of a society in which women are excluded from the crucial sphere of politics and are subordinated in the family. (Views of subordinate social groups typically contain contradictory elements, often affirming both the weakness and inferiority of those subordinated—traits which merit subordination—and their fearsome strengths—traits which require subordination). Rousseau's antifeminism is genuine and thorough; it is sharpened rather than qualified by his understanding of the power of women in familial and sexual relations.

Rousseau's views of women and the family are a crucial transitional point in the history of political theory. Rousseau deepened the critique of traditional patriarchal conceptions of the relations between the citizen and the state. In so doing, he cut the family off from political life even more sharply than did Locke in his critique of Filmer. At the same time, Rousseau's conception of the family and the subordination of women within it reached the limits of a radicalism organized around the integrity of the individual (male) citizen/proprietor.

Although Wexler's effort yields valuable insights

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into Rousseau's views, his approach is not adequate even as an alternative to ahistorical portrayals of Rousseau as a deeply radical theorist somewhat compromised by an antifeminism unrelated to the core of his thought.

DAVID PLOTKE
Socialist Revolution (San Francisco)

PROFESSOR WEXLER REPLIES:

I am grateful for Mr. Plotke's letter and his reading of my article.

If I understand him correctly, his chief objection to my interpretation of Rousseau's antifeminism is my failure to explain antifeminism as part of a larger social theory. Although I agree that I might have done more with the *Social Contract* and the second *Discourse*, I did show, in my discussion of the *Letter to d'Alembert*, how antifeminism was central to Rousseau's conception of an orderly body politic. What I thought needed explanation then, and what needs to be repeated now, is how *chacun à sa place* meant, for Rousseau, women in a very subordinate place socially and politically, and in an all-powerful place emotionally. There was no way of explaining this save by invoking some biographical considerations. I do believe that I have shown antifeminism to have been both at the core of Rousseau's political theories and at the bottom of his troubled heart.

VICTOR G. WEXLER
University of Maryland, Baltimore County

TO THE EDITOR:

I wish to discuss the "puzzles" and other charges made by Jonathan Spence in his review of my *Autocracy at Work* (*AHR*, 81 [October 1976]: 933–34). Surely the four aspects of my book that proved puzzling to the reviewer are but words or statements taken out of context. The Yung-cheng emperor's orders to emancipate the "mean people," for example, were unprecedented in Chinese history (pp. 20, 228–29). Although the emperor failed to achieve his goal, his efforts were still "epoch-making, politically and socially." (pp. 20, 230–31)

But the main point of Spence's review is that I have drawn without acknowledgment on two recent studies, the book by Silas Wu and the doctoral thesis by Kent Smith. Both studies appeared in 1970. Actually, I submitted my manuscript to Indiana University Press in 1968, and the manuscript was accepted early in 1970. Mainly because of a financing problem, it was not published until 1975. The book basically retains the same structure and conclusion of the 1968 manuscript. One can find my discussion on "administrative efficiency" in the manuscript (page 20) and also, in different wording, in two of my pre-1968 articles: "The Secret-Report System during the Yung-cheng Period" (in Chinese) and "Aspects of Ch'ing Autocracy." Both articles are cited in my book (pages 424 and 458 respectively).

In analyzing the palace memorial system and the Grand Council, I indicate, in two or three notes, the differences between my views and Wu's. These points are academic arguments, not "dismissive remarks," and they had already appeared in *Harvard Journal of Asiatic Studies* (31 [1971]: 323–32). More important, "dismissive remarks" do not provide evidence that I "seem to draw significantly on" Wu's book.

As for Kent Smith's thesis, I completed my chapter on Yung-cheng's ethnic policy in Southwest China in 1968. Fortunately, I was able to use his article, originally a panel paper for the 1968 convention of the Association for Asian Studies, which I attended. Though brief, the article was the foundation of Smith's dissertation. Besides, in my book I did not use any doctoral theses other than my own. My analysis was chiefly based on Chinese and Japanese sources, which I have surveyed, as Spence points out, "quite thoroughly." Moreover, the coverage is not identical. My chapter analyzes the formation and enforcement of policy in terms of Yung-cheng's autocratic rule. Smith's thesis is noticeably different, as suggested by its subtitle: "Aspects of Ortai's Governor-Generalship, 1726–1731."

The reader will be interested that the late Mary C. Wright evaluated my manuscript. She directed Spence's and Smith's dissertations, and was the tutor of Wu. In her press report in 1970 she wrote, "This [manuscript] is almost entirely an original scholarly work." She also stated that "two other scholars are working in fields which touch the subject of this book, Kent Smith and Silas Wu," and she did "not believe that either of these studies would offer competition of an undesirable kind once they reach the stage of publication."

PEI HUANG
Youngstown State University

CORRECTION:

In our recent review of a collection of essays edited by Stanley B. Winters and Joseph Held in collaboration with István Deák and Adam Wandruska (*AHR*, 82 [1977]: 143), part of the book's title was inadvertently omitted. The complete title is *Intellectual and Social Developments in the Habsburg Empire from Maria Theresa to World War I: Essays Dedicated to Robert Kann*. Our apologies to the editors and to Robert Kann.

THE EDITOR

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

WILLIAM O. AYDELOTTE, editor. *The History of Parliamentary Behavior*. (Quantitative Studies in History.) Princeton: Princeton University Press. 1977. Pp. xiv, 321. Cloth $22.50, paper $9.95.

DOUGLAS PRICE, Careers and Committees in the American Congress: The Problem of Structural Change. MOGENS N. PEDERSEN, The Personal Circulation of a Legislature: The Danish Folketing, 1849–1968. ANTOINE PROST and CHRISTIAN ROSENZVEIG, Measurement of Attitude Changes Among the Members of the French Chamber of Deputies, 1882–1884. GEOFFREY HOSKING and ANTHONY KING, Radicals and Whigs in the British Liberal Party, 1906–1914. AAGE R. CLAUSEN and SÖREN HOLMBERG, Legislative Voting Analysis in Disciplined Multi-Party Systems: The Swedish Case. PETER H. SMITH, The Making of the Mexican Constitution. WILLIAM O. AYDELOTTE, Constituency Influence on the British House of Commons, 1841–1847. DONALD R. MATTHEWS and JAMES A. STIMSON, Cue-Taking by Congressmen: A Model and a Computer Simulation: GUDMUND HERNES, Interests and the Structure of Influence: Some Aspects of the Norwegian Storting in the 1960s.

BRIAN BOND and IAN ROY. *War and Society: A Yearbook of Military History*. Volume 2. New York Holmes and Meier. 1977. Pp. 196. $11.00.

JOHN B. HATTENDORF, The American Navy in the World of Franklin and Jefferson, 1755–1826. V. G. KIERNAN, Colonial Africa and its Armies. NEIL A. WYNN, War and Social Change: The Black American in Two World Wars. SUZANN BUCKLEY, The Failure to Resolve the Problem of Venereal Disease among the Troops in Britain during World War I. ANTHONY R. WELLS, Staff Training and the Royal Navy, 1918–1939. WILLIAMSON MURRAY, German Air Power and the Munich Crisis. BRIGADIER F. H. VINDEN, The Introduction of War Office Selection Boards in the British Army: A Personal Recollection. STEPHEN BROOKS, Liddell Hart and his Papers. IAN ROY, The Army and its Critics in Seventeenth Century England. PETER PARET, Armed Forces and the State: The Historical Essays of Otto Hintze. BRIAN BOND, Aspects of War in the Nineteenth Century. REGINALD C. STUART, Clausewitz and the Americans: Bernard Brodie and Others on War and Policy.

RENATE BRIDENTHAL and CLAUDIA KOONZ, editors. *Becoming Visible: Women in European History*. Boston: Houghton Mifflin. 1977. Pp. x, 510. $6.95.

ELEANOR LEACOCK, Women in Egalitarian Societies. RUBY ROHRLICH-LEAVITT, Women in Transition: Crete and Sumer. MARYLIN ARTHUR, "Liberated" Women: The Classical Era. JOANN MCNAMARA and SUZANNE F. WEMPLE, Sanctity and Power: The Dual Pursuit of Medieval Women. E. WILLIAM MONTER, The Pedestal and the Stake: Courtly Love and Witchcraft. JOAN KELLY-GADOL, Did Women Have a Renaissance? SHERRIN MARSHALL WYNTJES, Women in the Reformation Era. RICHARD T. VANN, Toward a New Lifestyle: Women in Preindustrial Capitalism. ABBY R. KLEINBAUM, Women in the Age of Light. RUTH GRAHAM, Loaves and Liberty: Women in the French Revolution. MARY LYNN MCDOUGALL, Working-Class Women during the Industrial Revolution, 1780–1914. THERESA M. MCBRIDE, The Long Road Home: Women's Work and Industrialization. BARBARA CORRADO POPE, Angels in the Devil's Workshop: Leisured and Charitable Women in Nineteenth-Century England and France. EDITH F. HURWITZ, The International Sisterhood. BARBARA ENGEL, Women as Revolutionaries: The Case of the Russian Populists. BERNICE GLATZER ROSENTHAL, Love on the Tractor: Women in the Russian Revolution and After. TEMMA KAPLAN, Other Scenarios: Women and Spanish Anarchism. RENATE BRIDENTHAL, Something Old, Something New: Women Between the Two World Wars. CLAUDIA KOONZ, Mothers in the Fatherland: Women in Nazi Germany. ANDRÉE MICHEL, Family Models of the Future

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CHARLES F. DELZELL, editor. *The Future of History: Essays in the Vanderbilt University Centennial Symposium*. Nashville, Tenn.: Vanderbilt University Press. 1977. Pp. ix, 263. $13.95.

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JOHN BUTT and J. T. WARD, editors. *Scottish Themes: Essays in Honour of S. G. E. Lythe*. Foreword by Sir SAMUEL C. CURRAN. Edinburgh: Scottish Academic Press. 1976. Pp. xvi, 189. £5.00.

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COLIN BURGESS, Britain and Ireland in the Third and Second Millennia B.C.: a Preface. P. D. CATHERALL,

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STANLEY H. PALMER *et al.* *The Walter Prescott Webb Memorial Lectures: Essays on Modern European Revolutionary History*. Introduction by CHARLES TILLY. Edited by BEDE K. LACKNER and KENNETH ROY PHILP. Austin: University of Texas Press. 1977. Pp. xxiii, 132. $7.95.

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C. P. KINDLEBERGER, The Historical Background: Adam Smith and the Industrial Revolution. R. D. COLLISON BLACK, Smith's Contribution in Historical Perspective. THOMAS WILSON, Sympathy and Self-Interest. ALEXANDER CAIRNCROSS, The Market and the State. W. ARTHUR LEWIS, The Diffusion of Development. J. MARCUS FLEMING, Mercantilism and Free Trade Today. P. SYLOS-LABINI, Competition: The Product Markets. E. H. PHELPS BROWN, The Labour Market. JAMES M. BUCHANAN, Public Goods and Natural Liberty. R. A. MUSGRAVE, Adam Smith on Public Finance and Distribution. R. C. O. MATTHEWS, Public Policy and Monetary Expenditure.

FRITZ KLEIN, editor. *Studien zum deutschen Imperialismus vor 1914*. (Schriften des Zentralinstituts für Geschichte, number 47.) Berlin: Akademie-Verlag. 1976. Pp. 290. 25 M.

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ROBERT KANN *et al.*, editors. *The Habsburg Empire in World War I: Essays on the Intellectual, Military, Political and Economic Aspects of the Habsburg War Effort*. (East European Monographs, number 23.) Boulder, Colo.: East European Quarterly; distributed by Columbia University Press, N.Y. 1977. Pp. xiv, 247. $14.00.

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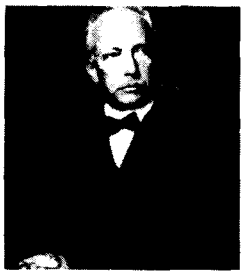
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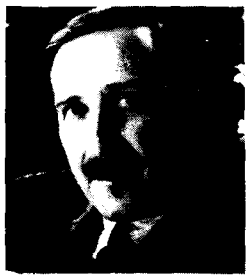
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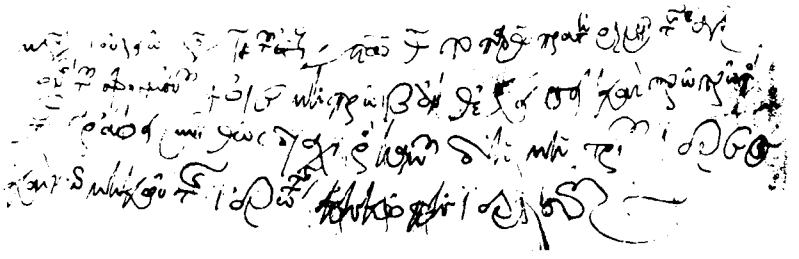
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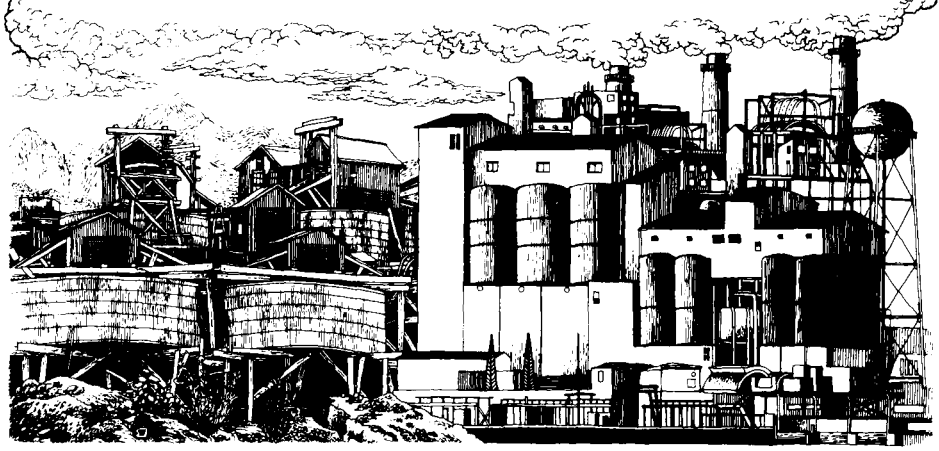
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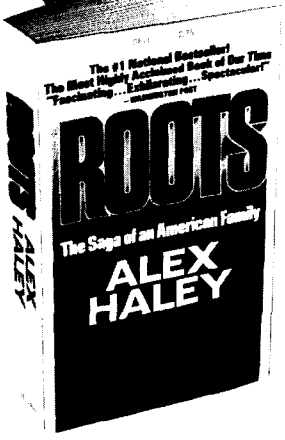
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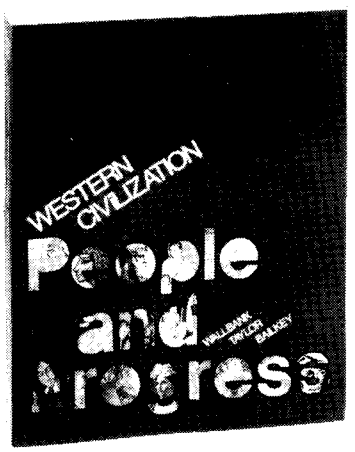
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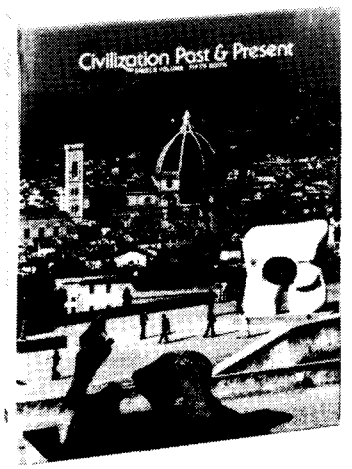
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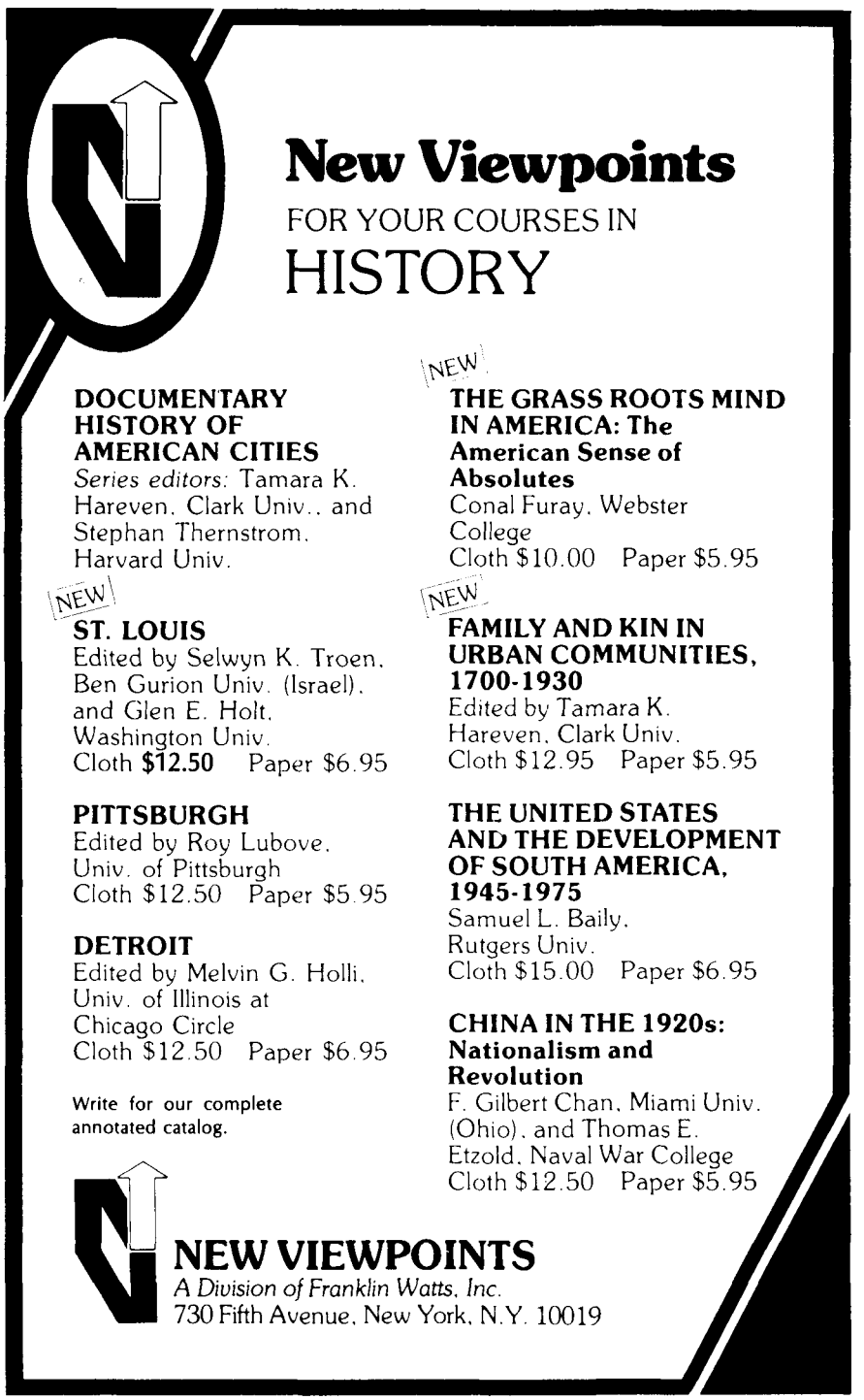
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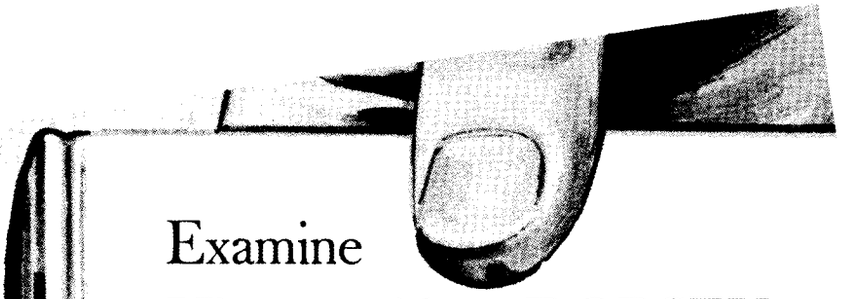
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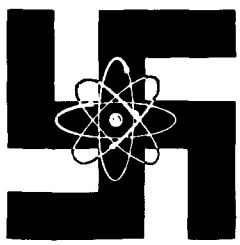
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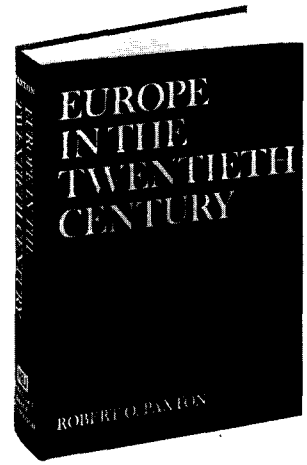
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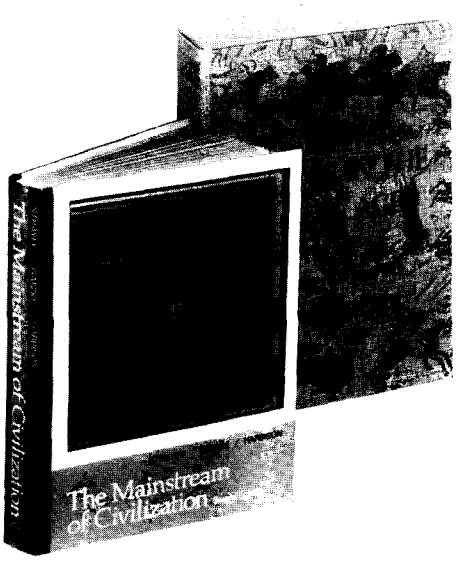


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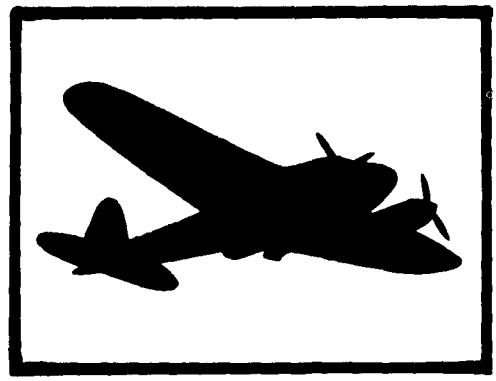
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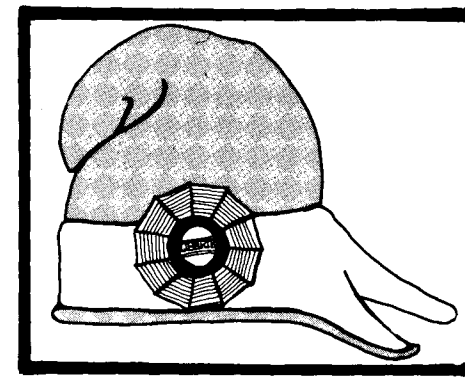
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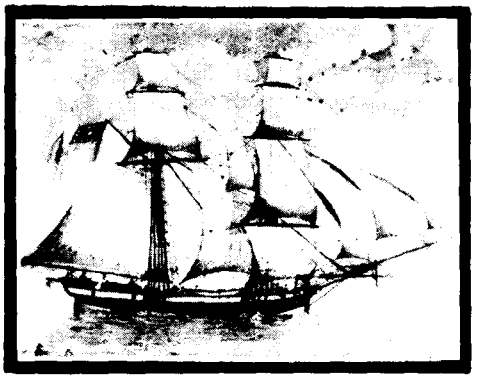
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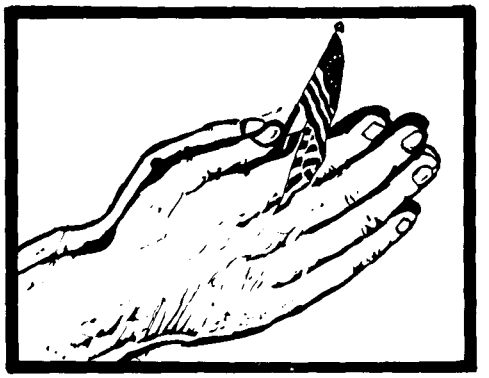
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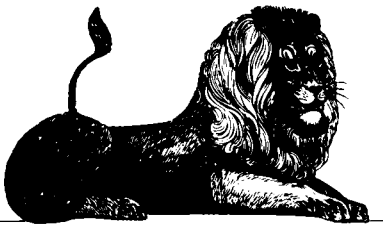
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